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Essays in Labor Economics and Child Welfare

A dissertation presented

by

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to

The Harvard Kennedy School

in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

in the subject of

Public Policy

Harvard University

Cambridge, Massachusetts

May 2025

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Essays in Labor Economics and Child Welfare

Abstract

This dissertation consists of three papers on topics in labor economics and child welfare.

Chapter 1 studies the rise of the nursing profession. In the early 20th century, hospitals founded training schools for nurses to meet the growing demand for medical care. Training schools increased overall nurse supply and soon became the primary pathway for young women to receive a professional nursing credential. I estimate how the availability of nurse training affected labor market outcomes. Using linked census records and training school openings as a source of variation, I show that white women who were geographically close to an opening in adolescence were more likely to become trained nurses. Effects are largest for women from well-off families, as proxied using father's occupation. Availability of nurse training caused women to substitute away from other occupations and had little effect on labor force participation or occupation-based measures of income. Furthermore, by their thirties, women who were geographically close to an opening were less likely to become physicians. These results paint a mixed picture: Nurse training provided new opportunities for women in the workforce, reinforcing existing gender segregation in medicine. Over the course of the 20th century, nursing would grow to become the largest majority-female occupation in the United States.

Chapter 2 studies state vaccination policy and child vaccination rates (co-authored with Samantha Gold). Vaccination rates have fallen below the threshold for herd immunity in many states, prompting concerns over exposure to infectious disease outbreaks. In this paper, we estimate how state efforts to enforce vaccination requirements for children in school have affected vaccination rates. In the past decade, several states have repealed non-medical vaccine exemptions. Using a staggered difference-in-differences design, we estimate that state repeal

raised kindergarten vaccination rates by 2 to 4 percentage points, with little substitution toward medical exemptions. We next examine whether school immunization audits further increased vaccination rates in California. Immunization audits were conducted starting in 2021 in schools with vaccine non-compliance rates of greater than 10 percent. Using a regression discontinuity design, we find no evidence of improved vaccine compliance in later years among schools at the audit threshold. Our results suggest that amidst growing vaccine hesitancy, state repeal of non-medical exemptions is particularly effective at keeping vaccination rates high. Furthermore, state policy has resulted in diverging vaccination rates between Democratic and Republican states. Based on our event study estimates, two-thirds of the gap in vaccination rates between Democratic and Republican states is driven by state exemption policy.

Chapter 3 studies the economics of foster care (previously published in the *Journal of Economic Perspectives* and co-authored with Joseph J. Doyle Jr., Max Gross, and Brian A. Jacob). Foster care provides substitute living arrangements to protect maltreated children. The practice is remarkably common: it is estimated that 5 percent of children in the United States are placed in foster care at some point during childhood. This paper describes the main tradeoffs in child welfare policy and provides background on policy and practice most in need of rigorous evidence. Trends include efforts to prevent foster care on the demand side and to improve foster home recruitment on the supply side. With increasing data availability and a growing interest in evidence-based practices, there are opportunities for economic research to inform policies that protect vulnerable children.

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Acknowledgments

I thank my advisors, Marcella Alsan, Christopher Avery, Peter Blair, and Claudia Goldin for their unwavering support. I have benefited greatly from their comments on early drafts and I am thankful for their encouragement. I thank the staff at Harvard's Center for Education Policy Research, in particular Ashley Dixon, and the team at Prince William County Public Schools for an excellent internship during my time as a PIER fellow. I thank the numerous other faculty and staff, including Thomas Kane, Joseph Doyle, and Nicole Tateosian, who provided valuable support during the Ph.D.

Finally, I thank my family and friends. This dissertation would not have been possible without your love and support, and for that I am deeply grateful.

To my mother, a lifelong nurse, and to all nurses, past and present, who dedicated their lives to bettering the health and well-being of others.

Chapter 1

The Birth of an Occupation: Professional Nursing in the Era of Public Health[1]

1.1 Introduction

The entrance of single and married women into the formal labor force was one of the largest economic shifts of the 20th century. The expansion of educational institutions, labor demand during wartime, and technological change all combined to gradually raise female labor force participation.[2] A key part of this transformation was the improvement of traditionally female jobs, in terms of on-the-job training, workplace conditions, and family-friendliness.[3] Yet we

[1] I thank Marcella Alsan, Christopher Avery, Peter Blair, Marianne Bitler, Amy Cross, Claudia Goldin, Brian Johnson, Samantha Batel Kane, Martin Karlsson, Savannah Noray, Bitsy Perlman, Kelsey Pukelis, Emma Rackstraw, Melissa Thomasson, Richard Zeckhauser, and participants in meetings of the Economic History Association, the Essen Health Conference, the Harvard Economic History Workshop, the Harvard Economics and Social Policy Workshop, and the NBER Summer Institute for thoughtful comments. This research was supported, in whole or in part, by the Institute of Education Sciences, U.S. Department of Education, through grant R305B150012 to Harvard University, and by the National Institute on Aging, through grant T32-AG000186 to the National Bureau of Economic Research. The opinions expressed here do not represent the views of the Institute, the U.S. Department of Education, the National Bureau of Economic Research, or Harvard University.

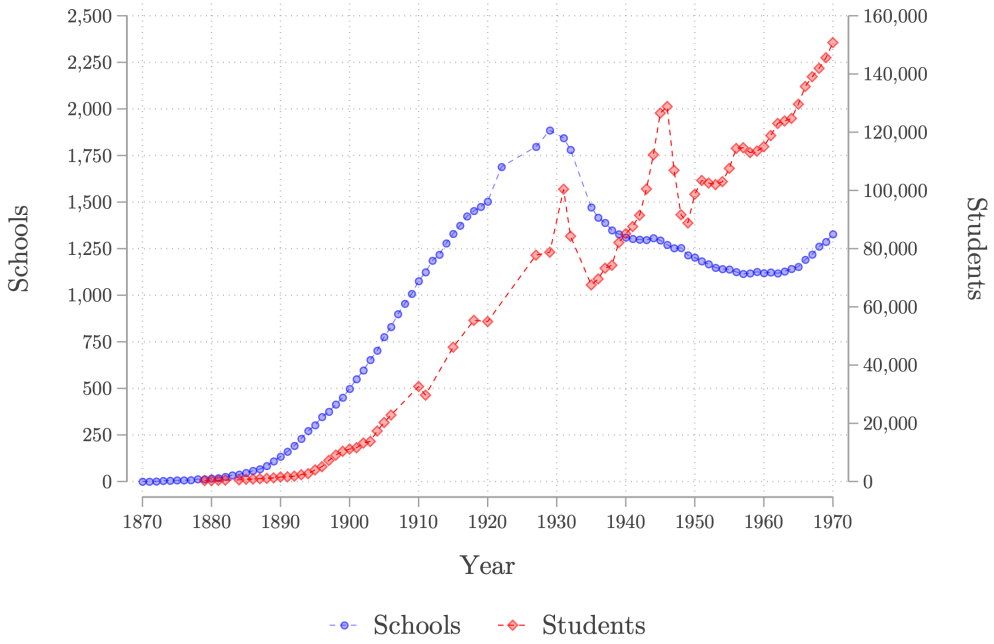
[2] For papers on educational opportunity and women's economic outcomes, see Goldin (1992), Goldin and Katz (2011b), Connolly (2021), and Bailey, Mohammed, and Mohnen (2025), among others. For papers on female employment during World War II, see Goldin (1991b), Goldin and Olivetti (2013), Jaworski (2014), Rose (2018), Shatnawi and Fishback (2018), and Brodeur and Kattan (2022). For papers on gender-biased technological change, see Vidart (2022) and Ager, Goñi, and Salvanes (2023).

[3] Kim and Tsao (2024) discuss how the removal of marriage bars in the 1930s increased labor force participation among married women. Goldin and Katz (2015) study the case of the pharmacist from the 1970s onward.

know relatively little about how such changes affected not just labor force participation, but also occupational choice, individual income, and the marriage market in the early 20th century.

This paper studies the establishment of training for nurses in the historical United States.[4] In 1870, hospital nursing required no credentials and employed primarily women from poor economic backgrounds. Inspired by Florence Nightingale's model of nurse training pioneered in England, U.S. hospitals organized training schools for nurses beginning in the 1870s. From 1880 to 1930, the number of schools grew from 15 to nearly 2,000 (Figure 1.1). This growth was driven by several factors, including the rise of nurse certification laws and the growing demand for medical care.

Figure 1.1: *Growth of nurse training schools and students*



Sources: Reports of the U.S. Bureau of Education, the American Nurses Association, and the Census Bureau. See Appendix A.3.
Notes: Figure plots the growth of nurse training schools (left axis) and nursing students (right axis) from 1870 to 1970.

[4] This paper draws on a large body of literature in history and nursing. Abel-Smith (1960) provides an overview of nursing in England. For nursing in U.S. history, see Rosenberg (1987), Reverby (1987), Hine (1989), Kalisch and Kalisch (2004), D'Antonio (2010), and Whelan (2021).

This paper first demonstrates that the spread of hospital-provided training raised the aggregate supply of professional nurses.[5] I use a newly constructed dataset of nurse training schools that captures openings between 1870 and 1920. Using a county-level event study, I show that training school openings increased the overall number of nurses, as well as the rate of nurses in the employed female population. Nurse supply was especially critical during the Era of Public Health. This period in U.S. history, from approximately 1890 to 1936, featured large-scale public health investments in the form of vaccination campaigns, school health initiatives, and hospital upgrades (Miller, 2008; Hoehn-Velasco, 2021; Hollingsworth et al., 2024).

This paper then turns to the availability of nurse training and labor market outcomes for women. In an era when few professional careers were open to women, nurse training schools offered entrance into a credentialed occupation and work in the burgeoning field of public health. However, several factors may have hampered the ability of women to benefit from a new form of training. Nursing was among the occupations that was not welcoming to married women in the first half of the 20th century (Goldin, 1991a). Many trained nurses left the labor force upon marriage. Furthermore, contemporaries were concerned about low-quality programs and compensation for nurses during economic downturns (Reverby, 1987; Whelan, 2021).

I use the geocoded dataset of nurse training schools to measure the precise distance to training for white women born in the U.S. between 1885 and 1900.[6] These birth cohorts experienced an increase in the availability of nurse training as schools opened across the country. To study labor market outcomes, I use cross-census links that follow women from childhood to adulthood, including women who marry and change surnames (Buckles, Haws, et al., 2023). I link roughly 50 percent of women observed in the 1900 census as children to the 1920 and 1930 censuses, when they were aged 20-35 and 30-45. For these women, I observe family background and location in 1900, as well as occupation and family outcomes in 1920 and 1930.

To identify causal effects, I leverage both distance to training and age. Women who were geographically close to a new nurse training school were more likely to take up nursing.

---

[5] Throughout this paper, I use the terms “professional" and “trained" interchangeably.

[6] Black women had access to sparse nurse training opportunities, primarily in black-run medical institutions. As nursing standards rose with the growth of training, it became harder for black women to receive training and have that training recognized (Hine, 1989). I limit the sample white American-born women primarily due to data limitations (see Appendix A.3).

Furthermore, training schools primarily admitted women between the ages of 18 and 21, and so an opening affected young women more than slightly older women in the same location. In a two-way fixed effects design, I identify treatment effects for women within five miles of a training school opening at ages 16 to 20 (in adolescence). Effects are relative to comparable women who were slightly farther away or slightly older.

In 1920, women who were geographically close to a school opening in adolescence were more likely to become trained nurses (an effect size of 20 percent relative to the sample mean). This effect is largest for women from well-off families, defined as having a father employed in a professional occupation. This finding aligns with qualitative evidence that training schools screened women based on education, religiosity, and social status (Rosenberg, 1987). Notably, I find limited evidence that access to training affected labor force participation or marriage in 1920. To enter nursing, women substituted away from a wide range of common occupations, such as teacher and bookkeeper. Finally, my analysis of economic returns to training is limited by the lack of wage data in the historical census. My approach is to construct occupation-based measures that capture absolute and relative economic status. Controlling for father's occupation, I find largely null effects of access to nurse training on economic status.

Women are observed in 1930 at ages 30-45, which allows for further analysis of labor market and family outcomes. Women who were geographically close to a school opening in adolescence were still more likely to be nurses in 1930, however, effects are smaller in magnitude. This is consistent with the fact that over 70 percent of women in the 1930 sample were married and no longer held paid employment. Relative to women who were less proximate to nurse training, I find that proximate women were more likely to be married and more likely to work as nurses conditional on marriage. I examine entry into other occupations and find that access to nurse training further reduced the likelihood of working as a physician. I interpret this finding as evidence that the expansion of hospital-provided nurse training reinforced existing gender segregation in the medical field. Similar to 1920, I find no evidence of improvements in economic status for women with relatively more access to nurse training.

This paper contributes to several strands of literature. I show how access to nurse training affected occupational sorting in a time when career options for women were limited. Single women found work in teaching, domestic service, or manufacturing, with few opportunities for

career advancement (Goldin, 1980, 1986b, 1990; Costa, 2000). Nursing was unique in that most leadership roles in the field were occupied by women.[7] I show that exposure to nurse training increased the likelihood of married women working in nursing, which was uncommon in the 1930s (Goldin, 1991a). This suggests that some nursing roles were flexible enough to accommodate married women, whose labor force participation did not significantly rise until later in the 20th century (Goldin, 2021).

This paper is the first to estimate the effects of a new form of workforce training in the early 20th century. Modern-day estimates suggest positive returns to vocational education and publicly-provided training, especially in the healthcare sector (Grosz, 2020; Silliman and Virtanen, 2022; Ng and Riehl, 2024; Xu et al., 2024).[8] We know less about returns in firm-sponsored settings, especially in historical contexts. I employ distance to schooling, which is commonly used as an instrument for general education (Card, 1995; Rouse, 1995; Kling, 2001; Cameron and Taber, 2004; Carneiro, Heckman, and Vytlacil, 2011; Nybom, 2017; Mountjoy, 2022).[9] I find that substitution across occupations is key to understanding the effects of nurse training on occupation-based measures of income. While I lack the wage data to estimate individual returns, this finding is similar to papers that examine returns to field of study (Altonji, Blom, and Meghir, 2012; Altonji, Arcidiacono, and Maurel, 2016; Kirkeboen, Leuven, and Mogstad, 2016; Lovenheim and Smith, 2022).

I identify a setting in which the spread of firm training shifted women into nursing from other common occupations. Broadly, occupation is determined by many factors, including perceived earnings, choice of college major, and non-pecuniary amenities, all of which can differ by gender (Zafar, 2013; Arcidiacono et al., 2020; Sloane, Hurst, and Black, 2021). Few prior papers have studied exogenous variation in occupation (Emran and Shilpi, 2011; Greenberg et al., 2024). Women in proximity to nurse training were less likely to work as physicians (a male-dominated occupation in 1930). This finding highlights training as one way that firms affect

---

[7] Some nurses spent entire careers in medicine and public health. Prominent figures include Clara Barton, Linda Richards, Mary Adelaide Nutting, Lilian Wald, and Mary Eliza Mahoney.

[8] A broader literature examines the returns to general education at different points in history (Griliches, 1977; Blundell et al., 1999; Goldin, 1999; Kane and Rouse, 1995; Feigenbaum and Tan, 2020; Clay, Lingwall, and Stephens Jr., 2021).

[9] My approach is also similar to papers that estimate effects of historical access to general education on outcomes like mortality and the production of notable people (Connolly, 2021; Doxey, Karger, and Nencka, 2022).

gender segregation, distinct from other channels (Goldin, 1986a; Card, Cardoso, and Kline, 2016). Gender segregation remains persistent today, both in medicine and in the broader labor market (Blau and Hendricks, 1979; Bertaux, 1991; Del Río and Alonso-Villar, 2015). The concentration of women in nursing contributes to the gender wage gap, as well as the gap in meaningful work (Turner and Bowen, 1999; Blau and Kahn, 2017; Burbano et al., 2023; Burbano, Padilla, and Meier, 2024). Today, nursing is the second most common four-year degree and one of the largest professional occupations.[10]

This paper proceeds as follows. Section 1.2 describes the historical background, the establishment of nurse training schools, and the supply of professional nurses. Section 1.3 provides estimates of how access to nurse training affected economic outcomes for women. Section 1.4 concludes.

1.2 The Birth of Professional Nursing

> *No single change transformed the hospital's day-to-day workings more than the acceptance of trained nurses and nurse training schools, which brought a disciplined corps of would-be professionals into wards previously dominated by the values and attitudes of working-class patients.*
>
> - Rosenberg (1987), *The Care of Strangers*, pg. 344

1.2.1 Historical context

Nurses, both male and female, have existed as caregivers throughout human history.[11] Religious orders routinely provided medical care for the impoverished. However, the concept of a "professional nurse"—a role in which one could receive formal medical training— was non-existent. In the mid-19th century, hospitals, many of them organized during wartime, relied on untrained stewards and attendants to care for patients (Whelan, 2021). Hospital wards employed

[10] Of the 1.2 million bachelor's degrees granted to women in 2021, 15 percent were in business and 11 percent were in registered nursing (NCES, 2021). In the 2022 American Community Survey, registered nurses made up 4.4 percent of employed women, the most common occupation code for this group. Labor force statistics throughout this paper are calculated from the census (Ruggles et al., 2022).

[11] The word "nurse" is derived from the Latin "nutrire", which means to suckle or derive nourishment. "Wet nurses", women who breastfed children who were not their own, were common through the end of the 19th century.

men and women from working-class backgrounds and provided no formal training. This was in part because worker attrition was substantial and education levels were low (Rosenberg, 1987).[12]

The Civil War in the U.S. and the Crimean War in Europe revealed the inadequacies of hospital organization, resulting in calls for reform. Florence Nightingale's approach to nurse training, pioneered in England in the 1860s, quickly became known to American hospital reformists (Abel-Smith, 1960). Nightingale's innovation was a year-long course in the art of nursing, holding trainees to high standards of discipline, cleanliness, and professionalism (Hawkins, 2010). The first U.S. training schools that followed the Nightingale model were established in the 1870s, including the Bellevue Training School in New York City, the Connecticut Training School in New Haven, and the Boston Training School in Boston.[13] Nightingale intended for these schools to be autonomous, free to train nurses for employment in nearby hospitals and private homes.

Demand for trained nurses was driven by the rising standards of modern medicine in the late 19th and early 20th century. Wealthy families preferred trained over untrained nurses to care for sick family members. The modern hospital required a trained nursing staff to uphold hygienic practices and to care for the growing number of middle-class Americans frequenting hospitals (Thomasson and Treber, 2008). Advances in medical technology increased the value of trained nurses by freeing physicians to learn new tools and perform more complex procedures. For instance, hospitals began stocking laboratory equipment to aid physician diagnoses in the 1880s.[14] During the Era of Public Health (1890-1936), large investments in infrastructure and

---

[12] In this period, the prevailing sentiment was that women were more suited for nursing than men, however, men still found work in hospitals and typically earned higher wages. We have little systematic evidence on hospital wages by gender in the pre-professional era. Rosenberg (1987) reports that at New York Hospital in 1840, the Board of Governors raised wages for male nurses in an effort to attract men of higher quality, but kept wages for female nurses the same and forbid them from earning outside income.

[13] A few hospitals had structured training programs prior to the 1870s. The Philadelphia Lying-in Charity began training nurses alongside physicians as early as 1832. Credit for the early founding of the Philadelphia Lying-in Charity is given to Dr. Joseph Warrington, who organized the training of nurses in obstetrics and general medicine (Huzza, 1887). In the 1860s, the Woman's Hospital in Philadelphia affiliated with Woman's Medical College to support the training of female physicians (Peitzman, 2000).

[14] Physicians may have also substituted toward leisure. This 1873 excerpt from the *Boston Medical and Surgical Journal* is revealing:

> *There is no reason why the long list of surgical dressings, the application of splints, etc., should not be done by trained nurses, and the hurried surgeon be given the opportunity of eating his dinner in comfort and getting to the polls at election time.* - Putnam (1874)

public health further increased demand for trained nurses in the community (Costa and Kahn, 2006).

1.2.2 Training school growth: Trends and explanations

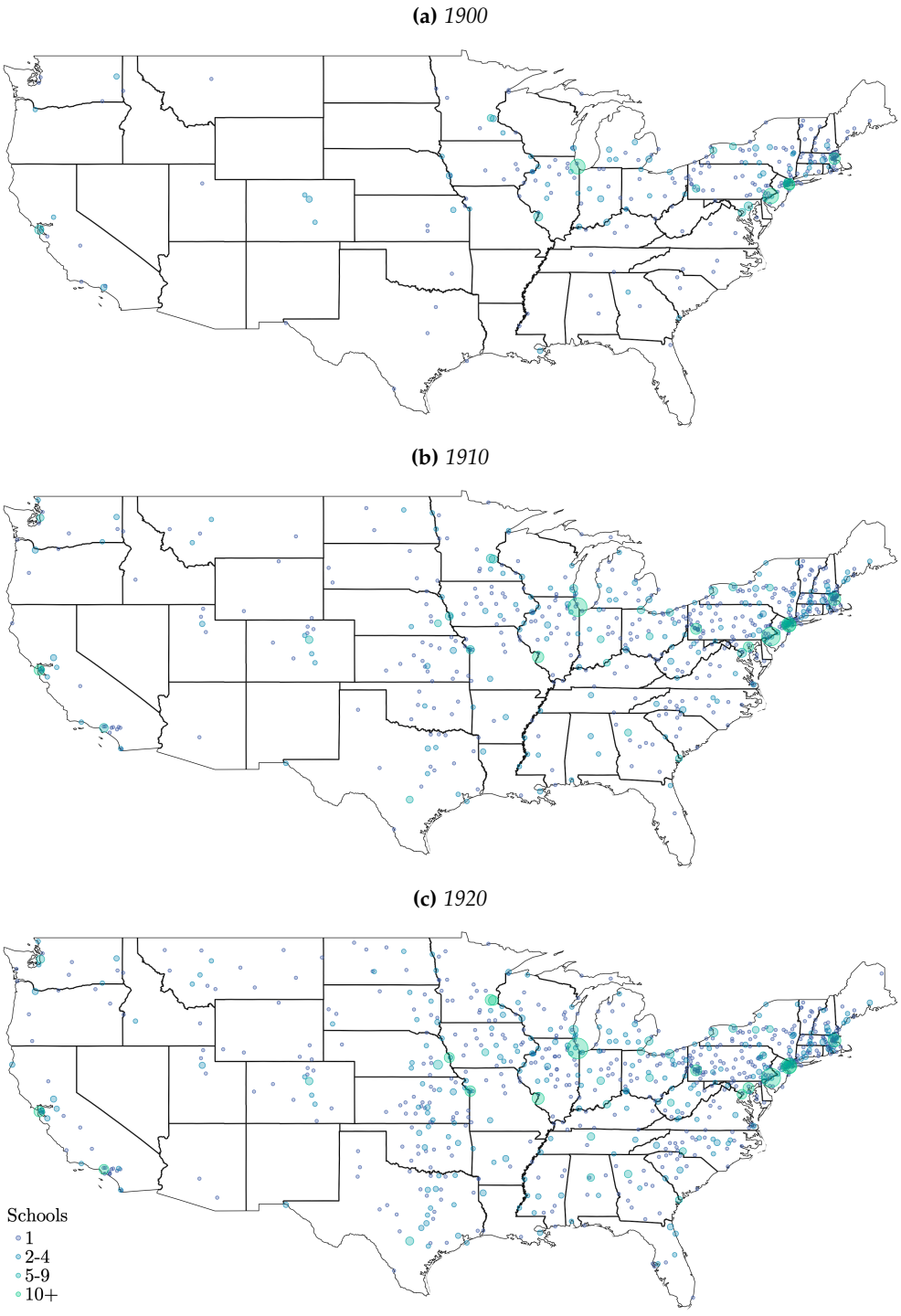
Dataset of nurse training schools

I construct a dataset of nurse training schools that were founded between 1873 and 1927. The data are compiled from reports published by the U.S. Bureau of Education and the American Nurses Association (henceforth the Bureau and the ANA).[15] The Bureau published reports that surveyed nurse training schools in the years 1879-1906 (excluding 1883), 1911, 1918, and 1927 (Nutting, 1912; US Bureau of Education, 1919, 1928). The ANA published two reports containing accredited training schools, one in 1918 and one in 1922 (American Nurses Association, 1918, 1922). Available information varies depending on the source and year. The most common fields recorded in the data are school name, city or town, year of organization, count of students (by gender), count of graduates, wages paid to students, and the length of training. All schools in the dataset are mapped to geographic coordinates (latitude and longitude) using the Census Place Project (Berkes, Karger, and Nencka, 2023). See Appendix A.3 for details.

Figure 1.1 plots the growth of nurse training schools and students. The total number of schools peaked in the late 1920s at just under 2,000 schools. Figure 1.2 plots the geographic distribution of nurse training schools in 1900, 1910, and 1920. Each point is a location, where point size is proportional to the number of schools. The earliest schools were affiliated with large urban hospitals, which explains the early concentration of schools in the Northeast. By 1900, 76 percent of the 100 most populous cities had a training school, compared to 63 percent of the next 100 populous cities. While some cities had ten or more training schools, many towns had just one by 1920.

[15] The Bureau was organized in 1867 and began publishing statistical reports on American education that year. The ANA was organized in 1896 as the first national organization for nurses.

Figure 1.2: *Locations of nurse training schools*



Sources: Reports of the U.S. Bureau of Education, the American Nurses Association, and the Census Bureau. See Appendix A.3.
Notes: Figure plots the locations of nurse training schools in 1900, 1910, and 1920. Counts are aggregated to the town level, and point size is proportional to the number of schools (see legend).

Why did hospitals train?

In the 1890s, hospitals began to adopt nurse training schools in large numbers. Why did this approach to training appeal to hospitals? There were obvious barriers, including capital costs (housing for nurse trainees). Hospital administrators also expressed concerns over giving training school leaders (who were women) autonomy in the hospital (Rosenberg, 1987; Comin and Hobijn, 2010). Hospitals could have simply allowed nurse training schools to exist independently, much like normal schools for teachers.

To understand why hospitals adopted this model, it is useful to turn to the canonical framework of firm-sponsored training popularized by Becker (1964). Under this framework, hospitals had no incentive to invest in *general* skills—in a competitive market, the hospital would have to pay trained nurses the market wage, so there was no way to recoup training costs. The hospital would rather the nurse arrive at their door already trained, with training provided by an outside entity. However, several departures from this framework made nurse training schools valuable. First, nursing involves a combination of *general* skills (e.g., changing linens, dressing wounds) and firm-*specific* skills (e.g., navigating the hospital ward, caring for specific types of patients). Hospitals may have decided to train because they could not unbundle general from specific training.

Second, we observe historical evidence that the nurse labor market was not perfectly competitive, which made it worthwhile for hospitals to train (Acemoglu and Pischke, 1999a,b). Information asymmetries existed: Hospitals privately observed the most productive students during the training period and hired them upon graduation (D'Antonio, 2010). The rest of the graduates entered the market for outside hospital jobs or private nursing roles. Many hospitals were the only formal medical institution in a given town. Furthermore, Moehling, Niemesh, and Thomasson (2019) shows how the Flexner report in 1910 resulted in the closure of medical schools that disproportionately admitted women. Migration to greater opportunity elsewhere was costly (Withrow, 2021). If a woman wanted to enter the medical field, the local hospital's nurse training program, which paid a small stipend, was often the least expensive option.

Professionalization of nursing

Nurse training schools offered not only training, but also a pathway to a credential and membership in professional organizations. States began to regulate the use of titles such as “registered nurse" (R.N.) in the first decade of the 20th century. In North Carolina, Mary Lewis Wyche, a nurse, is thought to have initiated the push for regulation by founding North Carolina’s State Nurses Association in 1902. In 1903, North Carolina passed a certification law requiring that all nurses who wished to practice under the title of “registered nurse" (R.N.) pass an examination administered by the state nursing board.[16] Successful applicants could practice as R.N. in any county in the state, and would be included in the county clerk’s register of trained nurses. Use of the title of R.N. without proper paperwork was punishable by a $50 fine or no more than 30 days imprisonment (Pollitt and Miller, 2010).

Within one year, New Jersey, New York, and Virginia followed the example of North Carolina. Figure A.4 plots the cumulative adoption of laws concerning registered nurses in the U.S. from 1890 to 1940, sourced from Carollo (2024). All states passed laws regulating nurses by 1940. Though certification was voluntary, advocates for reform viewed these laws as furthering the professionalization of nurses (Shannon, 1975). By contrast, midwifery was regulated by 20 percent of states in 1900 and 60 percent of states in 1940. Figure A.5 plots the year a certification law for registered nurses was adopted in each state. Early adopters were more likely to be mid-Atlantic states, and late adopters were more likely to be states that recently joined the Union.[17]

A prerequisite for certification was graduation from a state accredited nurse training school. State boards of nursing, which set standards and granted accreditation, were founded from 1880

[16] I distinguish between “certification" laws, which were generally voluntary, and “licensing" laws, which were mandatory and legally barred unlicensed individuals from practicing in an occupation. Many laws before 1940 did not require a license, but rather required an individual to obtain specific qualifications or certifications to use a protected title. These laws were also distinct from “registration" laws that require an individual to register in an occupation, without proof of competency (Carollo, 2024).

[17] In Table A.3, I identify the state characteristics that predict certification law adoption. Column (1) reports the results from a regression of adoption year on characteristics measured in 1900. Population and literacy emerge as statistically significant predictors, with larger and more literate states adopting earlier. In columns (2) and (3), I separately identify predictors of state adoption in the 1900s and the 1910s. The estimated coefficients largely align with column (1), though only literacy is statistically significant in these specifications. These findings broadly align with Carollo et al. (2022), who find that large labor markets and the presence of state associations predict the passage of licensing laws in many occupations.

to 1920, with most laws passing around 1910.[18] By tying the qualifications of nurses to the training hospital, the pathway to becoming a nurse became more formal and hospitals became further entrenched in the training process.

1.2.3 Characteristics of the nurse training school

> *The object of this Society is the training of nurses for the sick in order that women shall find a school for their education and the public shall reap the advantage of skilled and educated labor.*
>
> - Articles of Incorporation, Bellevue Training School for Nurses

Nurse training schools had two primary goals. The first was to educate young women in the medical sciences. Nurses attended lectures by training school staff and hospital physicians. Educational standards varied across schools, and the most prestigious programs included extensive courses in general medicine. For instance, in 1883, Massachusetts General Hospital provided lectures on diseases of women and children, surgical dressing, anatomy, anesthesia, and care of the newborn child. Such education would have been largely inaccessible to women outside of the nursing field. The professionalization of medical education during this period made it more difficult for women to train as physicians; Moehling, Niemesh, and Thomasson (2019) show that the proportion of female physicians fell from 1900 to 1940. Nurse training schools were therefore the primary venue for women to work in the medical sector. The second goal was to train student nurses to provide skilled nursing care to patients. To that end, student nurses spent much of their training in the wards of an affiliated hospital, sanitarium, or asylum. Hours were long, and as compensation, schools provided housing, uniforms, textbooks, and a small monthly stipend.

To ensure a minimum standard of care, training schools screened applicants based on educational background, physical health, and character.[19] Most schools required a short probationary period of one to three months. Several reports published by the Bureau of Education provide detailed information on nurse training school admissions. In the 1880s, the most common educational requirement was “a good common school education". High school was not explicitly

[18] It is revealing that 1910 was the year that the census separated “trained nurse" from “midwife" and “practical nurse" in its occupational classification.

[19] Prospective nurses were asked detailed personal questions and often needed references (Figure A.2).

mentioned, as this was decades before the high school movement took place (Goldin, 1998). Schools in 1880 listed the age range that they would consider, most commonly ages 22 to 35 (Figure A.6). It is unclear how binding these conditions were without detailed records from individual training schools. Regulations on child labor were uncommon before 1900, and so the minimum age for training would have been at the school's discretion (Hindman, 2002).

Over time, schools reported lower ages of admission and more precise educational requirements. At the median, the reported age minimum was 21 in 1896, falling gradually to 18 by 1927. To assess the actual working ages of nurses in training, I examine the age distribution of all women who received the 1950 occupation code for "student nurse". The distributions in each census from 1900 to 1930 are shown in Figure A.7. The majority of nurses were in training at ages 18 to 24, and I use this fact in my empirical strategy in Section 1.3.2. The left-ward shift in the distribution over time reflects the gradual adoption of lower age minimums. This shift was driven by several factors, including nurse shortages and school preferences (Reverby, 1987). The Bureau of Education did not report educational requirements again until 1911, when 24 percent of schools required a common school education, 39 percent required a high school diploma, and the remainder fell in between. By 1918, all schools required at least an 8th grade education. By 1927, the breakdown of required education was 2 percent requiring 8th grade, 64 percent requiring some high school, and 31 percent requiring high school. Very few programs listed postsecondary education as a requirement.

After spending two to three years in training (Figure A.8), graduates of nurse training schools could enter a number of roles in private or public health. In Appendix A.2, I discuss the employment and marriage patterns of graduates from three schools with digitized archival records: the Bellevue Hospital Training School for Nurses in New York City, the Presbyterian Hospital School of Nursing in Chicago, and the Massachusetts General Hospital School of Nursing in Boston. In 1900, the most common role for graduates was private duty nursing, which involved providing nursing care to sick individuals in their homes. As hospitals expanded during the early 20th century, nurses became more likely to work in hospitals or on the staff of training schools. Public health spending by states and cities led to the expansion of the public health nurse, infant nurse, and visiting nurse roles (Miller, 2008; Hoehn-Velasco, 2021). Finally, graduates of nurse

training schools often became certified, which allowed them to practice as registered nurses under state law.

1.2.4 Growth of professional nurses in the labor force

I characterize the growth of professional nurses in the decennial census, both in raw numbers and as a share of the labor force from 1870 to 2015. Tracking growth is challenging because the concept of a nurse did not stay constant over this long period. This resulted in changes over time in the subcategories assigned to nurses in the census. For instance, it was not until 1910 that practical (untrained) nurses received an occupation code to distinguish them from professional (trained) nurses. The 1950 coding of occupations allows us to partially overcome these challenges by partitioning nurses into subgroups based on industry and text from the original census enumeration. For the 1870-1940 period, I follow the 1950 coding and categorize nurses with an occupation code of 58 as professional nurses. For 1950-2015, I use the codes for professional nurse listed in Table A.7. For additional details on the samples and definitions used in this section, see Appendix Section A.3.2.

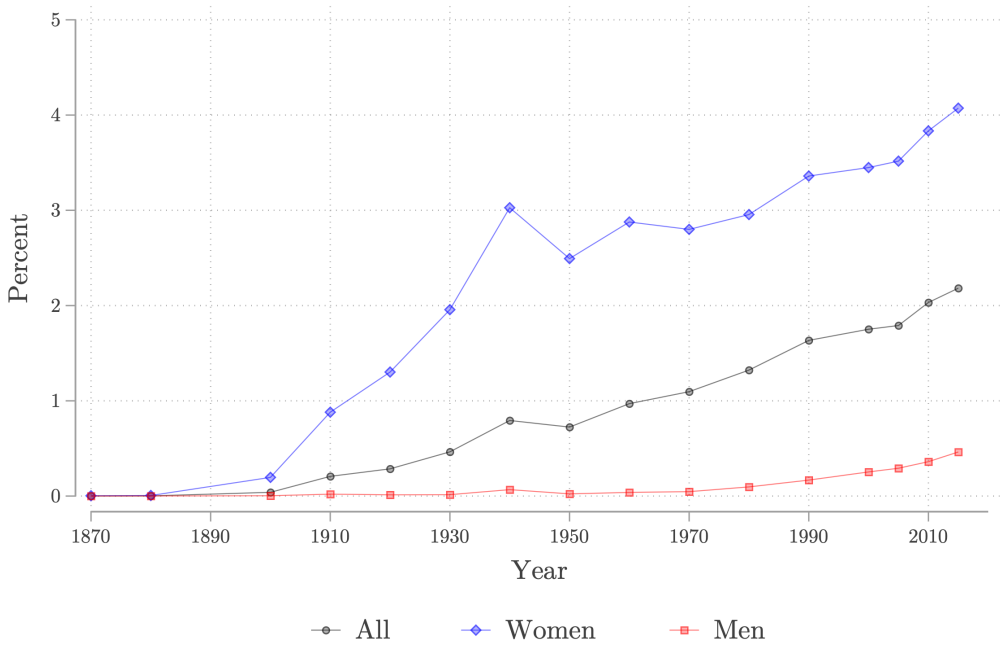
In 1870, the number of women coded as professional nurses ages 15-64 in the census was just 50. Following the growth of hospital-based medicine and the establishment of nurse training, professional female nurses in 1920 numbered over 100,000 (Table A.11). It is informative to measure growth of all nurses, including trained, student, and untrained nurses.[20] The total number of nurses increased from 9,000 to 230,000 during this same period. Figure 1.3 plots the percentage of the labor force employed in professional nursing from 1870 to 2015. From 1900 to 1930, professional nursing grew from less than 0.5 percent to 2 percent of the female labor force, and by 1930 the majority of nurses were counted as professional.[21] Overall growth slowed in

[20] Women may have received two other occupation codes: hospital attendant or midwife. These occupations were small relative to nursing: In 1880, there were 19,000 female nurses (Table A.11), relative to 900 hospital attendants and 2,000 midwives.

[21] Figure A.19 shows alternative measures of the percent of women and men working in nursing. Panel (a) shows that the share of women categorized as any type of nurse grew steadily to 4 percent in 1930. Panel (b) shows the rising share of men working in nursing since 1960.

the 1930s during the Great Depression, but growth has been steady since. In 2015, 4 percent of employed women worked in professional nursing.[22]

Figure 1.3: *Percent of the labor force in professional nursing*



Sources: Complete-count census for 1870-1940; 1% samples for 1950 and 1970; 5% samples for 1960, 1980, 1990, and 2000; American Community Survey for 2005, 2010, and 2015. Person weights are used for the 1950-2015 samples.
Notes: Figure shows professional nursing as a percent of the total labor force from 1870 to 2015. Percent in nursing by gender is also reported. The labor force is defined in Appendix Section A.3.2. Prior to 1950, "professional nurses" are individuals with a 1950 occupation code of 58. For 1950 onward, professional nurses are defined according to the codes in Table A.7.

Training schools and county nurse supply

Relative to a hospital with a fixed staff of nurses, hospital-affiliated nurse training schools generated a constant supply of newly trained nurses into the labor force. This suggests that training schools played a role in increasing the supply of nurses during the Era of Public Health.

[22] The growth of male and minority nurses is a recent development in the nursing field (Munnich and Wozniak, 2020). Prior to the era of professionalization, the field was more diverse, with male nurses and black nurses making up 5 and 20 percent of all nurses, respectively (Figure A.20, Panel (a)). The share of professional nurses who are male has increased to 10 percent in 2015 (Figure A.20, Panel (c)). In Figure A.20, Panel (b), I form a ratio of the percent in nursing and the percent in the overall labor force to assess each group's level of representation. A prominent U-shaped pattern emerges: representation of black female nurses fell from 1880 to 1940 as white women entered the profession in large numbers.

To formally investigate the contribution of training schools, I aggregate the total number of nurses in the complete-count census to the county-decade level from 1870 to 1920.[23] I estimate how the opening of the first training school in a given county affected nurse supply. The use of county will capture the change in nurse supply in the city with the new school, as well as any spillovers that occur to nearby towns within the county.

Supply y_{it} is measured in two ways: the log number of trained nurses and the rate of trained nurses per 1,000 individuals. The relevant population for nurse trainees was young white women. I therefore measure supply among employed white, native-born women aged 15-24. I report additional estimates using supply in a slightly older population: employed white, native-born women aged 25-44. This allows me to assess the impact of a training school opening on the supply of young nurses (many of whom were students at the time of the census) and slightly older nurses (previously trained, now employed).

I estimate the following event study specification:

$$y_{it} = \gamma_i + \tau_{u(i)t} + \psi X_{it} + \sum_s \mu_s D_{it}^s + \nu_{it}, \tag{1.1}$$

where y_{it} is the supply of nurses in county i and census year t. γ_i are county fixed effects. $\tau_{u(i)t}$ are state-by-census year fixed effects, where $u(i)$ is the respective state for county i. These fixed effects account for time-varying state factors that can affect nurse supply, such as certification laws. I include several time-varying controls in X_{it}: log of total county population, share of population urban, and share of population female. D_{it}^s is an indicator for whether county i has a nurse training school open s years ago. The event study coefficients of interest are μ_s, normalized relative to the census year just prior to treatment. On account of the decadal nature of census records, s is measured in decades before and after treatment, similar to Feigenbaum and Gross (2024). When supply is defined as a rate, the event study estimates are weighted by the size of the relevant population (the denominator). Standard errors are clustered at the county level.

In addition to the nonparametric event study in equation 1.1, I report parametric event studies and include a pretrend term that is linear in event time. The linear trend captures secular

[23] I end the panel in 1920, the last year that I can reliably measure school openings in the American Nurses Association (1922) report. County boundaries are from 2016, assigned to individuals via the Census Place Project (Berkes, Karger, and Nencka, 2023). Given that county boundaries can shift, this has the advantage of mapping individuals to a consistent geography over time.

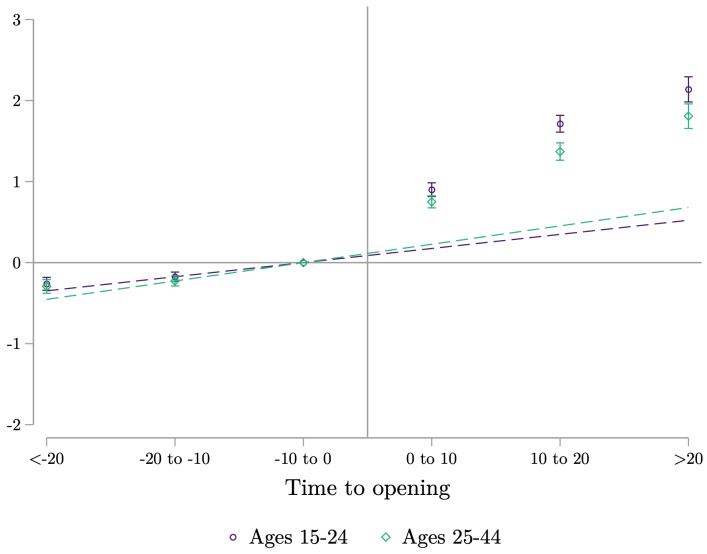
changes in the supply of nurses, which we might expect given the large time frame considered (several decades). To interpret deviations from the linear pretrend as causal, it is necessary that the timing of a training school opening be uncorrelated with other factors that might cause a trend deviation. In practice, I use the panel event study estimator from Freyaldenhoven, Hansen, and Shapiro (2019) and estimate a linear pretrend between periods -2 and -1.[24]

Figure 1.4 reports results for the log number of trained nurses. A training school opening in the average county increased the total number of trained nurses among both women aged 15-24 and women aged 25-44 (Panel (a)). This increase is apparent even after accounting for a linear pretrend (Panel (b)). Estimates for the 25-44 group are slightly smaller in magnitude, but still sizeable. Figure 1.5 reports similar findings using the rate of trained nurses. The magnitude of estimate is large: the pre-treatment mean for the supply of trained nurses is approximately 3 per 1,000. The detrended point estimate in Figure 1.5, Panel (b) suggests that training schools doubled the proportion of trained nurses within 10 years of opening. In Table A.10, I show that these estimates do not change substantially when using different sets of controls. Overall, the event study suggests that nurse training schools increased the supply of nurses in the population during a critical era of U.S. history.

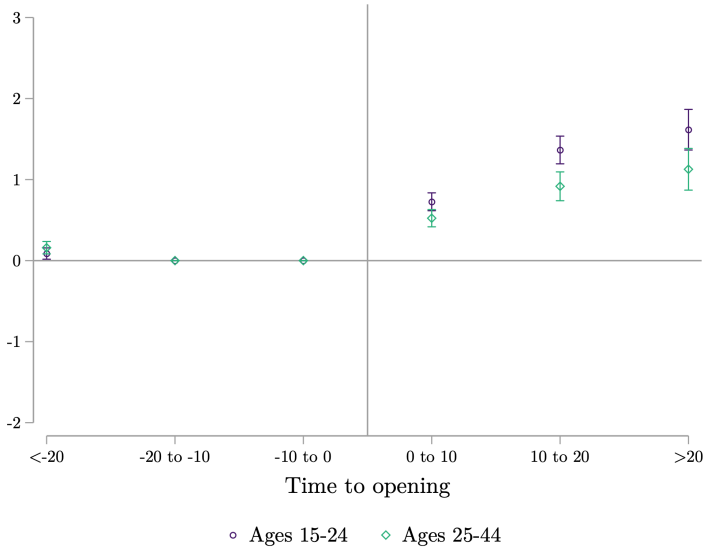
[24] See Dobkin et al. (2018) for an application that fits a linear pretrend using the entire pre-treatment period. Using this method does not change the results shown here.

Figure 1.4: *Effect of training school opening on log number of trained nurses*

(a) *Event study*



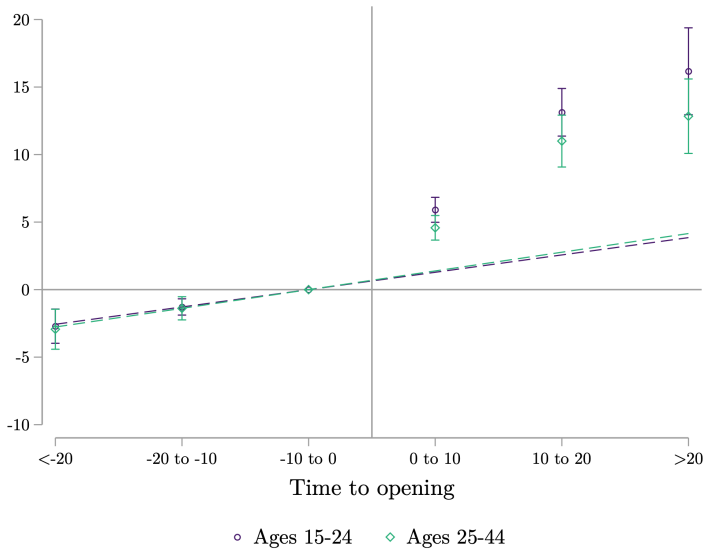
(b) *Event study (detrended)*



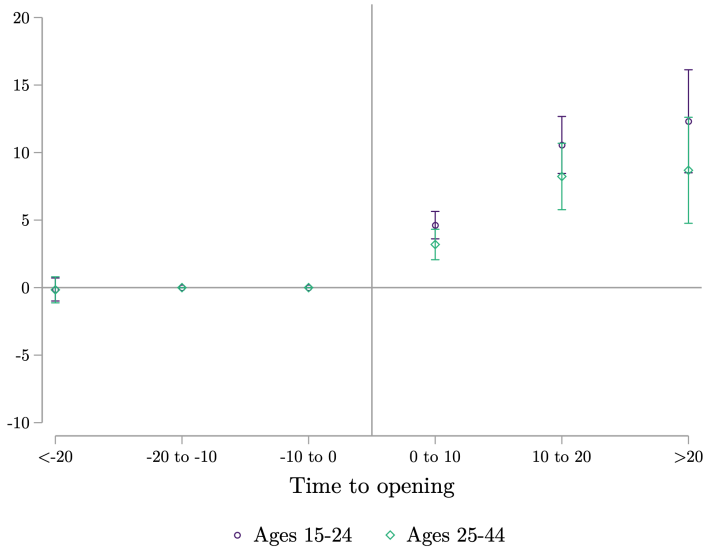
Notes: Figure shows event study estimates and 95 percent confidence intervals for the effect of training school opening on the log number of trained nurses. Results are shown for two populations: white native-born women ages 15-24 and ages 25-44. Panel (a) reports estimates of equation (1.1). Panel (b) reports estimates that adjust for a linear pretrend. Standard errors are clustered at the county level.

Figure 1.5: *Effect of training school opening on trained nurses per 1,000*

(a) *Event study*



(b) *Event study (detrended)*



Notes: Figure shows event study estimates and 95% confidence intervals for the effect of training school opening on the rate of trained nurses per 1,000 individuals. Results are shown for two populations: white native-born women ages 15-24 and ages 25-44. Panel (a) reports estimates of equation (1.1), weighted by the relevant population. Panel (b) reports estimates that adjust for a linear pretrend. Standard errors are clustered at the county level.

1.3 The Effects of Access to Nurse Training

Access to nurse training schools in the early 20th century was determined by a number of factors, such as distance from home. Distance represented both a monetary travel cost and a non-monetary cost of locating farther from family and established social networks. Non-monetary costs were likely larger for women due to gender norms: Withrow (2021) shows that white female migrants in the 1920s and 1930s were less likely to migrate long distances than white men. Qualitative evidence suggests that distance mattered in the case of nurse training. Reverby (1987) examines the birthplaces of nurses at three Boston-area training schools. She finds that for women who entered training from 1900 to 1919, 25 percent were from greater Boston and 25 percent were from elsewhere in New England. The concentration of local women aligns with literature that shows the relevance of distance for educational access in other settings (Card, 1995; Long, 2004; Doyle and Skinner, 2016; Walters, 2018; Acton et al., 2025).

The opening of a nearby nurse training school induced more women into training by lowering the costs associated with distance. Moreover, nurse training schools were cheaper than other forms of postsecondary schooling. A young woman could have attended a normal school for teachers, a college, or a university (one that admitted women), all of which charged tuition. Alternatively, nurse training schools provided housing and compensated students with a monthly stipend. The option to earn money while undergoing training was appealing, more so for women who were credit constrained (Withrow, 2021). Nurse training may have caused women to substitute away from careers with higher lifetime earnings but larger upfront costs (Hanushek et al., 2017). Motivated by this, I examine whether the effects of nurse training school proximity differ by economic background, as proxied by parental occupation in the census.

Access to nurse training mattered differently for women of different age, race, and family background. As shown in Section 2.2, nurse training schools primarily admitted women ages 18 to 24 while they were still unmarried and without children. This made economic sense from the perspective of the hospital, but may have also made sense for the women themselves. For a woman 25 or older, switching into nursing may have been costly, especially if she had already married or left the labor force. Black women were excluded from nearly all nurse training schools regardless of background or age (Hine, 1989). Much of the rhetoric around nursing during the

early 20th century was focused on the shift in social status. The installment of structured training legitimized nursing as a profession and raised standards significantly. Women from higher-status families may have had an easier time meeting these standards.

Along with employment in nursing, access to nurse training may have affected overall labor force participation and entrance into alternative occupations. Training and obtaining a nursing credential may increase overall labor force attachment by shifting attitudes or connection to work (Fernández, 2013). Regarding alternatives to trained nursing, women may have upskilled by substituting away from untrained medical occupations. Teaching is often compared to nursing as a respectable path for women to earn independent income during this period. Historical evidence suggests that among women who worked before entering a nursing program, many had worked in teaching, clerical, or healthcare jobs (see Appendix Section A.1.2).

1.3.1 Sample construction

The main analysis sample contains women in the 1900 complete-count census who were born in the U.S. between 1885 and 1900.[25] These women came of age during the growth of training schools (Figure 1.1), which makes them ideal candidates for understanding the effects of nurse training. Furthermore, women in this sample were less than 15 years old in 1900, and the vast majority were living with their parents. This proves useful for two reasons. First, parental characteristics (such as father's occupation) can be readily constructed. Second, location in 1900 can be used to measure access to nurse training, before work or migration decisions are made.[26] Location in 1900 comes from the Census Place Project, which assigns individuals in the census to a standardized location identifier, latitude, and longitude (Berkes, Karger, and Nencka, 2023). From this location,

[25] The 1900 census was enumerated on June 1. The destruction of the 1890 census means that 1900 is the first year that we observe these cohorts in surviving census records.

[26] One challenge faced in prior literature is that precise location in childhood is not always observable. This can pose a problem for proximity-style instruments, sometimes termed "endogenous mobility bias". For instance, Currie and Moretti (2003) use mother's location at first birth to estimate effects of proximate college openings on health. If this location is endogenous (e.g., women move to areas with more openings, for educational or non-educational reasons), then instrumental variables estimates may be biased. To circumvent this issue, Connolly (2021) uses a combination of census and Social Security Numident death records to identify town of birth. The Numident records have poor coverage of individuals who died before 1970 and therefore likely exclude a substantial portion of the 1885-1900 birth cohorts.

the distance to nearby training schools is then geocoded as a straight-line distance (in miles). See Section 2.3 and Appendix A.3 for details on the dataset of nurse training schools.

To study the effects of access to nurse training on later-life outcomes, the 1900 sample is linked to the 1920 and 1930 censuses. This linkage is necessary to observe both geographic location in childhood and labor market outcomes in adulthood.[27] The 1920 sample is observed at ages 20 to 35, which allows for the measurement of labor force and marriage outcomes early in life (when many women were still of training age). The 1930 census captures outcomes for the linked cohorts at ages 30-45, after all education has taken place. This allows for an exploration of whether effects persist (or newly emerge) relative to 1920.[28]

The cross-census links are derived from the FamilySearch genealogy platform (Price et al., 2021; Buckles, Haws, et al., 2023).[29] The majority of women marry and change surnames, which precludes the use of traditional linking methods that rely on first and last name (Abramitzky et al., 2021). FamilySearch users can draw on a variety of sources (birth, marriage, and death records, as well as private knowledge) to construct detailed family trees. Buckles, Haws, et al. (2023) use these trees to train an XGBoost machine learning algorithm and form additional links. By combining these links with other linked panels, such as the Multigenerational Longitudinal Panel, Buckles, Haws, et al. (2023) construct over 300 million links for women.[30]

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[27] Without this linkage, the only information on birth location for women in the 1920 or 1930 censuses is birth state (or country). Prior studies have used state of birth and year of birth to estimate returns to schooling using state-level variation in compulsory schooling laws, without the need for cross-census links (Acemoglu and Angrist, 2000; Lleras-Muney, 2002; Stephens and Yang, 2014; Clay, Lingwall, and Stephens Jr., 2021). This approach will incorrectly assign laws for some individuals who move states during childhood. Alternatively, Goldin and Katz (2011a) use contemporaneous high school enrollment to estimate effects of compulsory schooling laws. To identify location during schooling age, Moorthy, Figinski, and Lloro (2022) use longitudinal records from the Health and Retirement Study to match individuals to state of residence at age ten.

[28] The use of the 1920 census is not without its limitations. It was enumerated in January, in the wake of World War I and the Spanish flu epidemic. Additionally, a few questions were not asked in 1920, including duration of marriage, number of children ever born, and children surviving. I use the number of children in the household to examine family size, but cannot include age at first marriage as an outcome for this sample.

[29] As an alternative, Espín-Sánchez, Ferrie, and Vickers (2023) and Althoff, Gray, and Reichardt (2024) link women over time using Social Security Administration (SSA) records matched to census. These records are primarily made up of Social Security Number (SSN) applications that record place of birth, date of birth, father name, and mother (maiden) name. While these linked samples have the advantage of containing place of birth, sample sizes using this method are generally smaller than comparable samples in FamilySearch.

[30] For more information, see here: https://censustree.org.

The 1900 sample contains 11,420,000 native-born white women. Of these, 6,331,000 (55 percent) are linked to the 1920 census via FamilySearch. These links include married women in 1920, who make up 60 percent of the linked sample. While this linkage is necessary to observe adult outcomes, it affects the interpretation of the results by limiting the sample. Following Bailey et al. (2020), I construct propensity scores for the likelihood of linkage using observable characteristics in 1900 (see Appendix Section A.3.2 for details). The construction of the 1900-1930 linked analysis sample is similar to that of the 1900-1920 sample. Of the 11,420,000 native-born white women in the 1900 sample, 5,530,000 (48 percent) are linked to the 1930 census via FamilySearch. This sample is not limited to women also observed in the 1920 sample. The primary reason is to improve power by maximizing sample size.

1.3.2 Empirical approach

I use a two-way fixed effects strategy to estimate the effects of *access* to nurse training. Women who grew up in close proximity to nurse training were likely different on many dimensions (for instance, they were more likely to be located in a dense urban center). To estimate causal effects, I leverage the fact that nurse training schools opened across the country beginning in the 1870s (Figure 1.1). These openings shifted access to nurse training, allowing for a comparison of women who were in close geographic proximity to a new school with women who were slightly farther away. Furthermore, as discussed in Section 2.2, schools typically admitted women between the ages of 18 and 21. Women who were relatively young at the time of an opening can be compared to women who were relatively older and were less likely to take up training.[31]

Formally, consider woman i born in year t and observed in location j in 1900 as a child. Let y_{ijt} be the outcome for woman i observed in adulthood (for instance, employment as a trained nurse). Define Near_j to be an indicator for whether location j was less than five miles from a nurse training school opening (where miles is geocoded using straight-line distance). Let Age_{jt} be an indicator for whether a woman was aged 16 to 20 at the time of the opening. I estimate the

[31] This approach is similar to papers that use treatment "intensity" at different ages as a source of identifying variation (Duflo, 2001; Baker, Blanchette, and Eriksson, 2020; Kose, Kuka, and Shenhav, 2021).

following two-way fixed effects specification:

$$y_{ijt} = \theta_j + \delta_{c(j)t} + \beta \text{Near}_j \times \text{Age}_{jt} + \Gamma \mathbf{X}_i + \varepsilon_{ijt}. \tag{1.2}$$

The treatment effect of interest is β, the effect of proximity to a training school opening (distance less than five miles) at ages 16 to 20. θ_j is a set of fixed effects for childhood location in 1900, which captures time-invariant differences across places. For instance, choice of occupation might have been driven by differences across towns in proximity to large cities or historical presence of specific industries. Recall that the location indicators are fine-grained and capture differences across small geographies, such as suburbs outside a major city. $\delta_{c(j)t}$ is a set of birth year-by-county fixed effects, which controls for differences in outcomes across birth cohorts. This term also captures county-level changes that affect cohorts differently, such as implementation of suffrage laws or local labor regulations. $\mathbf{X}_i$ is a vector of individual characteristics measured in 1900 for woman i that may determine labor market outcomes. The following characteristics are included: indicators for father/mother presence, indicators for father/mother literacy, father/mother occupation scores, indicators for father/mother employment in medicine, and family size.

To arrive at the final analysis samples, I restrict to women within ten miles of a training school opening at ages 16-25. Restricting the sample to women within ten miles of an opening drops women from small, rural areas that were not in close proximity to any nurse training schools. This restricts the comparisons to “treated" women who were within five miles of an opening and “control" women who were slightly further away (5-10 miles). This approach is sometimes referred to as the “ring method" in studies that identify effects of spatial treatments (Linden and Rockoff, 2008; Currie et al., 2015, 2024). Furthermore, by only including women who experienced openings at ages 16-25, the age comparison for the treatment group (ages 16-20) is to slightly older women (ages 21-25) who were less likely to take up training. While this restriction reduces sample size, it serves to make the underlying analysis sample more homogeneous and the comparisons in equation (1.2) more precise.

Table 1.1 reports descriptive statistics. Characteristics of childhood town, father, and mother are measured in the baseline year of 1900, and adult characteristics are measured in 1920. Column (1) reports statistics for the full 1900-1920 linked sample, restricted to women with non-missing geocoded location (N = 6,004,123). Columns (2) and (3) report statistics for the final analysis

sample (N = 1,646,657), split by treatment status (e.g., whether a woman was less than five miles from a training school opening at ages 16-20). Relative to the full sample in column (1), treated women in column (2) are located in larger towns in the Northeast. They were also from families with a higher economic status in 1900, in terms of parental literacy and occupational income. By limiting the full sample to women within ten miles of an opening at ages 16-25, the sample of untreated women appears more similar to treated women (column (3)). In 1920, the marriage rate for women in the sample was roughly 50 percent. Conditional on working, 2 percent of women in the sample worked in a nursing occupation. Table A.12 reports descriptive statistics for the 1930 sample. The final sample size is 1,326,420, slightly smaller than in 1920. The 1930 sample is largely similar to the 1920 sample in terms of observable characteristics, with the exception of marriage and employment. Whereas 40 percent of the sample were employed in 1920, just 25 percent were employed in 1930. This drop in employment is consistent with the fact that over 70 percent of women in the 1930 sample were married.

As tests of identification, equation (1.2) is estimated with baseline characteristics measured in 1900. This is akin to running placebo tests with the potential confounding variables as the outcome (Pei, Pischke, and Schwandt, 2019). These specifications retain all fixed effects (θ_j, $\delta_{c(j)t}$), but do not include individual controls $\mathbf{X}_i$. The results from these tests for the 1920 sample are shown in Table 1.2. The treatment measure, $\text{Near}_j \times \text{Age}_{jt}$, does not predict significant differences in baseline family size, parental literacy, parental occupation score, or whether a parent is employed in a medical occupation.[32]

1.3.3 Short-run results: 1920

Employment and occupation

I first report results for the linked 1900-1920 sample, when the women were ages 20 to 35. Table 1.3 reports results from equation (1.2), estimated using OLS with standard errors clustered at the town level. All estimates use inverse propensity score weights to adjust for the likelihood of linkage (Bailey et al., 2020). Coefficients have been multiplied by 100 and can be interpreted in percentage

[32] In Table A.13, I run the same set of placebo tests for the 1930 sample. I find null results for all outcomes except mother's occupation score. The magnitude of this imbalance is small, and all main specifications control for background characteristics. Note also that p-values are unadjusted for multiple hypothesis testing.

Table 1.1: *Descriptive statistics, 1920*

	(1) Full sample	(2) Treated = 1	(3) Treated = 0
Childhood town, 1900			
Ln(population)	8.36	11.74	9.90
Region = North	0.28	0.41	0.53
Region = South	0.42	0.41	0.28
Region = Midwest	0.25	0.13	0.15
Region = West	0.05	0.05	0.04
Father characteristics, 1900			
Literate	0.93	0.96	0.95
Employed	0.91	0.87	0.89
Occupation score | Employed	21.55	27.78	24.58
Mother characteristics, 1900			
Number of children	4.17	3.93	4.08
Literate	0.93	0.95	0.93
Employed	0.04	0.04	0.04
Occupation score | Employed	15.57	17.92	16.95
Adult characteristics, 1920			
Married	0.60	0.50	0.53
Employed	0.29	0.41	0.37
Nurse | Employed	0.03	0.02	0.02
Occupation score | Employed	21.88	23.02	22.60
Unique places	33,217	1,907	4,581
Observations	6,004,123	972,710	673,947

Notes: Table reports descriptive statistics for the 1920 linked sample, described in Section 1.3.1. Statistics are weighted using inverse propensity scores to adjust for the likelihood of linkage. Column (1) reports means for the full sample. Column (2) reports means for treated women (e.g., women who were within five miles of an opening at ages 16-20). Column (3) reports means for the remainder of the final analysis sample. Father and mother characteristics are for women with an identifiable father or mother in the 1900 census. Nurse is an indicator for work in any nursing occupation. Occupation score and nurse variables are conditional on gainful employment.

points. The outcome in column (1) is an indicator for whether a woman was employed in any nursing occupation in 1920. In Panel A, results are shown for the full analysis sample. Women who were within five miles of a training school opening at ages 16 to 20 were 0.13 percentage points more likely to work in nursing. This coefficient is statistically significant at the 1 percent level and is not sensitive to the addition of individual-level controls (column (2)). Relative to the overall sample mean of 0.83, this represents a 16 percent increase in the likelihood of entering nursing. Recall that these estimates can be considered “reduced form" effects of access to nurse training at ages 16-20, since the 1920 census measures occupation and not actual training school enrollment. The goal of training schools was to train women in professional nursing. As shown in

Table 1.2: *Placebo tests with father and mother characteristics, 1920*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Father characteristics			Mother characteristics			
	Literate	Occ score	Medicine	N children	Literate	Occ score	Medicine
Opening <5 mi. at ages 16-20	0.052	0.063	0.019	−0.012	0.046	−0.029	−0.007
	(0.114)	(0.057)	(0.036)	(0.009)	(0.119)	(0.020)	(0.005)
Controls	No	No	No	No	No	No	No
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	95.62	23.32	0.65	3.99	94.60	0.68	0.04
Observations	1,526,425	1,526,425	1,526,425	1,585,412	1,585,412	1,585,412	1,585,412

Notes: Table reports results for equation (1.2), estimated using OLS. Placebo outcomes are father and mother characteristics from the 1900 census: *Number of children* (as reported by the mother), *Literate* (father/mother can read and write), *Occ score* (father/mother occupation score), and *Medicine* (father/mother works in medicine). Coefficients on binary variables (*Literate* and *Medicine*) are multiplied by 100 and can be interpreted in percentage points. The sample is described in Section 1.3.1. Sample is restricted to women with an identifiable father (columns (1)-(3)) or mother (columns (4)-(7)). All specifications include town fixed effects and birth year-by-county fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses.

column (3), women were 0.10 percentage points more likely to be observed as a trained nurse (19 percent relative to the sample mean of 0.54). The subcategory of practical nurse likely captures women working in either untrained or informal nursing roles. As shown in column (5), effects on practical nursing are small and insignificant. None of the estimates are sensitive to adding individual-level controls. This suggests that the entire effect in column (1) is driven by entrance into trained nursing, with little substitution away from practical nursing roles.

The estimates in Table 1.3, Panel A include a substantial number of women who were not working. In Table A.14, I examine whether my proximity to training measure causes an increase in gainful employment (having a recorded occupation in 1920). I do not find strong evidence of effects on this margin: the coefficient of 0.20 percentage points is positive but not statistically significant. I conclude that access to nurse training did not increase employment among women who otherwise might not have worked. However, the coefficient of 0.20 is large enough to include the overall increase in nursing employment observed in Table 1.3.

In the following analysis, I examine entrance into nursing conditional on gainful employment. While the effect on employment is statistically insignificant, conditioning on an outcome can cause endogenous selection bias (Elwert and Winship, 2014). I report results with this caution in mind. Results are shown in Table 1.3, Panel B. Point estimates indicate that women were 0.45 percentage

Table 1.3: *Effects of proximity on entrance into nursing, 1920*

	(1)	(2)	(3)	(4)	(5)	(6)
			Subcategories			
	Nurse		Trained nurse		Practical nurse	
Panel A: Full sample						
Opening <5 mi. at ages 16-20	0.126***	0.126***	0.103***	0.103***	0.023	0.023
	(0.048)	(0.048)	(0.037)	(0.037)	(0.029)	(0.029)
Controls	No	Yes	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	0.83	0.83	0.54	0.54	0.28	0.28
Observations	1,646,657	1,646,657	1,646,657	1,646,657	1,646,657	1,646,657
Panel B: Employed						
Opening <5 mi. at ages 16-20	0.447***	0.449***	0.342***	0.342***	0.105	0.106
	(0.151)	(0.151)	(0.116)	(0.116)	(0.094)	(0.094)
Controls	No	Yes	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	2.08	2.08	1.37	1.37	0.71	0.71
Observations	655,027	655,027	655,027	655,027	655,027	655,027

Notes: Table reports results for equation (1.2), estimated using OLS. The dependent variable in column (1) is an indicator for working in a nursing occupation in 1920. Columns (3) and (5) reports effects for the subcategories of trained and practical nurse. Columns (2), (4), and (6) report results with individual controls added. Coefficients are multiplied by 100 and can be interpreted in percentage points. The sample is native-born white women, as described in Section 1.3.1. Panel A reports results for the full sample. Panel B reports results for women who are gainfully employed. All specifications include town fixed effects and birth year-by-county fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses. $^{***}p < 0.01$.

points more likely to work in a nursing occupation (column (1)), with most of the effect driven by trained nursing (column (3)). Relative to the sample mean of 1.37 percent, white women who were within five miles of a training school opening when they were ages 16 to 20 were 25 percent more likely to work as trained nurses. As in Panel A, the results for practical nursing conditional on employment are positive and not statistically significant, and the estimates do not change with the addition of individual controls.

I consider the effect of proximity to nurse training on entrance into other health occupations in Table A.15. First, hospital or physician attendant was one occupation that could have been affected by the opening of a nurse training school. This group was small (just 0.21 percent of employed women) and I do not see significant effects of proximity (column (1)). While other health professions, such as physician, make up a small fraction of women, it is possible that nurse training acted as a gateway to higher-status health professions. Alternatively, nursing may have reinforced the pathway for women into medicine and diverted them from medical schools, especially during

the medical school closures of the 1910s (Moehling, Niemesh, and Thomasson, 2019; Clay et al., 2023). Column (2) shows that female physicians make up just 0.06 percent of employed women in the sample, and proximity to nurse training has no effect on becoming a physician (point estimates are negative). Results are similarly null for the broader group of professional health occupations: physicians, dentists, osteopaths, and pharmacists (column (3)). In column (4), I find an insignificant effect of proximity on the set of semi-professional health occupations that includes chiropractors, dietitians, medical technicians, therapists, and midwifes. Entrance into other health occupations may have been hindered by the spread of licensing regulations at the state level during this period. For instance, midwifery was a regulated occupation by the 1920s (Anderson et al., 2020).

Robustness

I examine the robustness of the coefficient on nursing in several ways, shown in Figure A.21. The main point estimate in Table 1.3, column (2) is shown in red. I show that the main coefficient is robust to specifying different end points for the birth cohorts I consider in my main analysis sample. Adding foreign-born women (N = 45,000) has no effect, however, note that this is a highly selected sample of foreign-born women who were present in the 1900 census while young. I assess whether the weighting scheme matters. The main coefficient is not sensitive to using an unweighted estimator or dropping the top 1 percent of weights. I also estimate an alternative set of weights that is based on an entirely female census sample (equivalent to fully interacting gender with all other predictors of linkage), and this does not change the main results.

Heterogeneity analysis

I estimate effects of proximity to nurse training for different groups. First, I examine selection into nursing by family background. Rosenberg (1987) highlights the interaction between nurse training and class background, writing how training filled the wards with "would-be professionals".[33]

[33] Other historical sources have a similar theme. Nutting (1912) quotes a physician at Johns Hopkins, who writes:

> *As long as nurses were drawn from the lower classes and as long as the training was merely manual and not intellectual, there could be no profession of nursing. Now that nursing is offering ever new incentives, the position of the trained nurse has become more elevated, the educational standards are high, the time of nursing*

Family characteristics are measured in 1900, and include parental occupation and occupational income. I proxy for class background by splitting the sample into three groups based on father's occupation. The groupings are sourced from the occupational scheme of Song et al. (2020): (1) professional/managerial occupations (e.g., doctors, lawyers, managers), and (2) routine/manual occupations (e.g., clerks, craftsmen, miners), and (3) farming occupations. The sample contains roughly 200,000 women in group 1, 1,000,000 women in group 2, and 200,000 women in group 3.

Table A.16 reports the estimates on entrance into nursing for each group. In Panel A, women from professional families are 0.51 percentage points more likely to work as a trained nurse (a 78 percent increase relative to the mean). This is a large increase in relative magnitude, surpassing the size of the coefficient for employed women. The effect of exposure does not appear to operate through any other healthcare profession (including practical nursing). Compared to Panel A, Panel B shows an increase in the likelihood of working in nursing, but the coefficient is much smaller for this group. Panel C shows that women from farming families were not entering nursing despite their close proximity.[34] This suggests that nurse training was primarily an opportunity for relatively higher-status women. Note that these estimates are reduced-form in nature and I cannot examine who applied to nurse training schools. As discussed in Section 2.2, standards were high, and so women from less well-off backgrounds may have been screened out at the application stage. These results demonstrate that training schools were a mediating factor in elevating the status of nursing, which is evidence in line with Rosenberg (1987).

Next, I examine how effects vary by marital status. Nurse training schools often would not admit married women, choosing instead to admit young single women. Once a woman graduated from training school, she often continued working up until marriage.[35] The departure from nursing upon marriage may not have been a choice: Nurse training emerged during a period when married women were frequently fired (Goldin, 1991a). Given that half of the sample is

> *has become long enough to permit of thoroughness without overtaxing, the material position of nurses is being improved, the opportunities for higher careers in nursing are multiplied, we find the profession appealing more and more to the best class of woman.*

[34] In a fully interacted specification including all three groups, the coefficient on the treatment interacted with group 1 (father - professional) is statistically different from group 2 (p-value = 0.093) and group 3 (p-value = 0.019).

[35] In Appendix A.2, I conduct a case study of training school graduates and show that, conditional on marriage, employment among graduates was very low. For women in their twenties in 1910 and 1920, only 9 and 3 percent of graduates were working while married, respectively (Appendix Table A.5).

married, I have adequate power to assess differences by marital status. Note that I examine whether exposure to training affected marital status in Table A.14, and I find a positive and insignificant coefficient of 0.17 percentage points (Panel A). Additionally, I do not see strong effects on overall employment for single or married women (Panels B and C). Point estimates are positive for married women and negative for single women, but standard errors are relatively large.

Tables A.17 and A.18 report results for single and married women, respectively. For single women, the effect of proximity on employment in nursing is 2.5 times larger than the baseline estimate in Table 1.3. Women who were less than five miles from an opening at ages 16 to 20 were 0.34 percentage points more likely to work in nursing, an increase of 21 percent relative to the baseline mean (Panel A). Most of this effect is driven by entrance into trained nursing, whereas the coefficient on practical nursing is positive but not statistically significant. Conditional on employment, women were 0.53 percentage points more likely to work in nursing (an increase of 24 percent relative to the baseline mean). As with the full sample, I do not find evidence that other health occupations were affected (columns (4)-(7)). For married women, in Table A.18, Panel A, I find insignificant effects of proximity to nurse training on work in nursing.[36] The overall rate of work is low (7.5 percent), therefore the gainfully employed sample in Panel B is small. Conditional on employment, the coefficient on nursing is 0.392 (statistically insignificant). The magnitude, however, is sizeable relative to the mean. Additionally, columns (3) and (4) suggest that some married women were working as practical nurses rather than attendants. I view this as suggestive evidence that trained nurses in 1920 had a limited ability to work in formal nursing roles while married, which aligns with historical evidence (Goldin, 1991a; Kalisch and Kalisch, 2004).

Occupation-based measures of economic status

I examine how access to nurse training affected measures of economic status. As discussed in Appendix Section A.3.2, the census does not contain wage information before 1940, so I employ

[36] In a fully interacted specification including single and married, the coefficient on the treatment interacted with marriage is statistically significant (p-value = 0.005).

occupation-based income measures commonly used to study historical inequality and mobility.[37] I focus on occupational income and the Song score, which captures an occupation's relative status based on human capital content (Song et al., 2020; Ward, 2023; Buckles, Price, et al., 2023). Conditional on working, changes in economic status are determined by sorting across occupations.

In Table 1.4, I show effects of proximity to nurse training in adolescence on the log of occupational income score and LIDO score. In the full sample (Panel A), non-working individuals are assigned a score of zero. As shown in columns (1) and (2), the average effect of proximity is positive but statistically insignificant. Nursing lies in the middle of the occupational income distribution (Figure A.17), so we might not expect a strong positive effect unless women were entering employment or substituting from low-income occupations. In Panel B, I report results conditional on employment. I add the Song score as an outcome since this is a relative percentile measure. Across all measures, I find no significant positive effects. Similar to the LIDO score, the Song score for nurses relative to other common occupations is slightly lower (Figure A.18).

I conduct several exercises to understand the estimates in Table 1.4. First, I examine how proximity to nurse training affected substitution from other common occupations. In Table A.19, I report results for the occupations of teacher, clerk, bookkeeper, secretary, and laborer (including farm laborer). These five occupations cover approximately 50 percent of the employed women in the sample. In both the full and employed samples, I find mostly negative and insignificant coefficients.[38] This suggests that women were substituting into nursing and away from a wide range of occupations, rather than just one (such as teaching).[39]

Second, I examine heterogeneity in the effects on economic status by family background in Table A.20. Conditional on employment, columns (1) and (2) report positive but insignificant effects on absolute measure of income. Despite the strong positive effect of proximity on entrance into nursing for women with professional fathers in Panel A, I estimate no gain in economic

[37] Prior to 1940, census enumerators asked about literacy and schooling status as a binary indicator. Some earlier datasets do contain information on education and wages. The 1915 Iowa state census is the largest such dataset, which prior studies have used to estimate returns to education and economic mobility (Goldin and Katz, 2000; Feigenbaum, 2018).

[38] In results not shown, I examine heterogeneity in occupational substitution by marital status, and results are similar.

[39] In Appendix Section A.1.2, I discuss historical evidence that nurses worked previously in teaching, clerical, or health occupations. This only captures women who worked before starting nursing, whereas I capture substitution for all women, including those for whom nursing was their first job.

Table 1.4: *Effects of proximity on occupational income, 1920*

	(1) Ln(occ. score)	(2) Ln(LIDO score)	(3) Song score
Panel A: Full sample			
Opening <5 mi. at ages 16-20	0.009	0.006	
	(0.007)	(0.006)	
Controls	Yes	Yes	
Fixed effects	Yes	Yes	
Mean outcome	1.25	1.01	
Observations	1,646,657	1,560,844	
Panel B: Employed			
Opening <5 mi. at ages 16-20	−0.003	−0.003	0.280
	(0.003)	(0.004)	(0.229)
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Mean outcome	3.13	2.77	74.79
Observations	655,027	569,214	653,990

Notes: Table reports results for equation (1.2), estimated using OLS. The dependent variables are the natural log of occupation score, the natural log of LIDO score, and Song score (see Appendix Section A.3.2). The sample is native-born white women, as described in Section 1.3.1. Panel A reports results for the full sample. Panel B reports results for women who are gainfully employed. All specifications include individual controls, town fixed effects, and birth year-by-county fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses.

status. In column (3), the only significant increase in relative status (Song score) is for women with fathers in routine and manual occupations (Panel B). This coefficient is in percentiles and represents a less than 1 percent increase relative to the subgroup mean.[40]

These results suggest that nursing did not result in meaningful gains in economic status, measured in either absolute or relative terms. The primary limitation is that real wage income is not observed in 1920. Additionally, non-monetary benefits to nurse training are not captured by this analysis. Nursing is a highly prosocial and meaningful occupation, and many nurses report that these factors are important reasons for choosing the occupation. Nurse training then, much like today, taught medical knowledge and caregiving skills. These skills can be used to improve the health and well-being of family members. These benefits provide one reason why women were willing to substitute away from other occupations to pursue training as a nurse.

[40] In results not shown, I find no effect of proximity on employment and marital status in each subgroup. I also look at occupation score and LIDO score in the full sample and find insignificant results. Replicating Table A.19 for each subgroup, I find largely null results, however, women from well-off backgrounds are less likely to become teachers relative to the other subgroups.

1.3.4 Long-run results: 1930

Employment and occupation

I report results for the 1900-1930 linked sample, when the women were ages 30 to 45. The sample is smaller, but allows for more power to study married women, who make up 70 percent of the analysis sample. This analysis can inform whether availability of training played a role in increasing labor force participation, which was rising among married women during this period (Goldin, 1983).

Table 1.5 reports results for nursing in 1930, analogous to the 1920 results reported in Table 1.3. Effects on nursing in the full and employed samples (Panels A and B) are less than half the size of the effects in 1920 and no longer statistically significant. As in 1920, I examine whether women exposed to nurse training are more or less likely to be employed or married in 1930 (the extensive margin). These results are reported in Table A.21. Effects on employment are negative but not statistically significant with controls (column (2)). In column (3), women are 0.6 percentage points more likely to be married by 1930 (0.8 percent increase relative to the sample mean), which likely drives a portion of the negative coefficient observed on employment. The coefficient on marriage has several interpretations, according to related literature on history and nursing. For instance, Kalisch and Kalisch (2004) point out that with the lowering of training age from 1900 to 1920, it was easier for a woman to train and subsequently marry in her twenties.

I examine substitution among other health occupations in 1930, shown in Table A.22. The estimates in Panel A suggest that women exposed to nurse training are 0.02 percentage points less likely to become physicians (column (2)), with no significant effect on other occupations. The coefficient is sizeable given that only 0.04 percent of women were physicians in 1930. Note also that the effect on physician in 1920 is negative but not statistically significant. It is possible that 1920 was too early to fully capture effects on the physician occupation given that many women were still of training age. I interpret this result as evidence that the professionalization of nursing via training contributed to gender segregation within medicine.[41]

[41] Moehling, Niemesh, and Thomasson (2019) find reductions in medical school enrollment for women during this period.

Table 1.5: *Effects of proximity on entrance into nursing, 1930*

	(1)	(2)	(3)	(4)	(5)	(6)
			Subcategories			
	Nurse		Trained nurse		Practical nurse	
Panel A: Full sample						
Opening <5 mi. at ages 16-20	0.058	0.059	0.035	0.036	0.023	0.023
	(0.051)	(0.051)	(0.039)	(0.039)	(0.033)	(0.033)
Controls	No	Yes	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	0.82	0.82	0.56	0.56	0.26	0.26
Observations	1,326,420	1,326,420	1,326,420	1,326,420	1,326,420	1,326,420
Panel B: Employed						
Opening <5 mi. at ages 16-20	0.371	0.373	0.186	0.187	0.184	0.185
	(0.231)	(0.231)	(0.175)	(0.175)	(0.155)	(0.155)
Controls	No	Yes	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	3.16	3.16	2.16	2.16	1.00	1.00
Observations	346,121	346,121	346,121	346,121	346,121	346,121

Notes: Table reports results for equation (1.2), estimated using OLS. The dependent variable in column (1) is an indicator for working in a nursing occupation in 1930. Columns (3) and (5) reports effects for the subcategories of trained and practical nurse. Columns (2), (4), and (6) report results with individual controls added. Coefficients are multiplied by 100 and can be interpreted in percentage points. The sample is native-born white women, as described in Section 1.3.1. Panel A reports results for the full sample. Panel B reports results for women who are gainfully employed. All specifications include town fixed effects and birth year-by-county fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses.

Heterogeneity analysis

In Table A.23, I report results for married women in 1930.[42] In Panel A, column (1) shows that married women who were proximate to nurse training in adolescence were more likely to work in nursing. Much of this increase is driven by practical nursing (column (3)). I interpret this result as an increased attachment to the field of nursing, as it suggests these women were working in less formal nursing roles. In 1930, as in 1920, nurses who married were uncommon, so work in practical nursing might have been a more feasible option. Note also that married women were less likely to become physicians, and this pattern of results holds conditional on employment (Panel B). In Table A.21, I show that married women were not more likely to be gainfully employed, which suggests that the increase in nursing operates through the intensive margin.

[42] In results not shown, I estimate analogous results for single women. Since only 28 percent of the 1930 sample are single, the standard errors for these results are large. Results show increases in the likelihood of working as nurses (coefficient of 0.10) and decreases in the likelihood of working as physicians (coefficient of -0.05), but coefficients are not statistically significant.

Occupation-based measures of economic status

I study effects of proximity on measures of economic status, as in Section 1.3.3. The approach for 1930 is the same as for 1920, and includes the same measures (occupation score, LIDO score, and Song score). In Table 1.6, women who were proximate to nurse training in adolescence have marginally lower occupation scores. In Panel A, all non-employed women receive a value of zero, so the negative coefficient on employment in Table A.21 is driving this result. In Panel B, results are insignificant and negative across all measures, which suggests that occupational sorting did not improve economic status for employed women in 1930. However, the earlier caveat with the 1920 sample remains that actual wage income is not observable.

Table 1.6: *Effects of proximity on occupational income, 1930*

	(1) Ln(occ. score)	(2) Ln(LIDO score)	(3) Song score
Panel A: Full sample			
Opening <5 mi. at ages 16-20	−0.011*	−0.005	
	(0.007)	(0.006)	
Controls	Yes	Yes	
Fixed effects	Yes	Yes	
Mean outcome	0.82	0.68	
Observations	1,326,420	1,292,775	
Panel B: Employed			
Opening <5 mi. at ages 16-20	−0.002	−0.004	−0.115
	(0.005)	(0.004)	(0.295)
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Mean outcome	3.13	2.81	74.87
Observations	346,121	312,476	344,313

Notes: Table reports results for equation (1.2), estimated using OLS. The dependent variables are the natural log of occupation score, the natural log of LIDO score, and Song score (see Appendix Section A.3.2). The sample is native-born white women, as described in Section 1.3.1. Panel A reports results for the full sample. Panel B reports results for women who are gainfully employed. All specifications include individual controls, town fixed effects, and birth year-by-county fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses. *$p < 0.10$.

These results are incomplete without a discussion of husband's characteristics. For the married women in the sample, nurse training may have affected economic status through the marriage market. I test whether married women exposed to training schools were more likely to marry relatively higher-earning men in 1930. These results are reported in Table A.24. In

column (1), I do not find increases in husband's occupational income score. In column (2), I find increases in the likelihood of having a husband working in a medical occupation (e.g., physician, pharmacist), however, the coefficient is not statistically significant. I conclude that training has limited effects on measures of economic status, either through the labor market for women who were employed, or through the marriage market for women who were married.

1.4 Conclusion

The growth of training for nurses is emblematic of the shift of caring labor from informal to formal settings, as well as the broader entrance of women into the formal labor force. I use the spread of nurse training schools over time to estimate how training affected nurse supply and the economic outcomes of women born at the turn of the 20th century. First, I show that women who had relatively more access to training were more likely to become nurses in their twenties. This effect is driven by women from well-off families. Second, I find limited evidence that access to nursing improved economic status or increased labor force participation. While this analysis is limited by the lack of historical wage data, the results imply that the women most likely to become nurses had equally or higher-earning alternative occupations. Third, I show evidence of fade out in the effects of employment in nursing. By 1930, effect sizes are smaller given the propensity for women to marry and drop out of the labor force. However, I show some evidence that married women were more likely to stay working in nursing, which foreshadows the coming rise of married women in the labor force. Finally, women proximate to nurse training were more likely to be married and less likely to work as physicians, which I interpret as evidence that nurse training reinforced gender segregation within the medical field.

Since the establishment of nurse training, nursing has grown into one of the largest occupations among women today. Over 1 in 25 women work as professional nurses, the largest single occupation group for working women. The supply of new nursing degrees is also large: nursing ranked second in terms of bachelor's degrees granted to women in 2021. In contrast to nursing in 1870, nursing today is highly respected. According to Gallup, nursing was the highest ranked career in terms of honesty in 2022, a position it has held for the past 20 years (Gallup Analytics, 2022). I show that training affects selection into nursing, yet a broader question remains:

How does the social standing of a profession change over time and across contexts? And does this standing affect who chooses to enter?

Furthermore, this paper leaves open several directions for research on workforce training. Black and foreign-born women may have benefited from the spread of new vocational training opportunities at the turn of the 20th century. Workforce training may have generated local spillovers, even if private returns to training were not significant. In particular, the supply of nurses may have helped save lives, especially during times of crisis like the 1918 Spanish flu pandemic. Finally, new opportunities for training are constantly emerging as industries develop in the modern-day economy. More evidence is necessary to understand the long-run effects of this training and credentials on different types of workers.

Chapter 2

State Enforcement of School Immunization Requirements and Child Vaccination Rates[1]

2.1 Introduction

Child vaccination rates in the U.S. are on the decline (Figure 2.1). In 39 states, the measles vaccination rate for kindergartners has fallen below 95 percent, a commonly cited threshold for herd immunity (Fine, Eames, and Heymann, 2011). A direct result of falling vaccination rates is that young children today are increasingly vulnerable to infectious disease outbreaks (Omer et al., 2009). Despite the fact that the Centers for Disease Control and Prevention (CDC) declared measles eliminated in 2000, notable outbreaks have recently occurred in several states. In 2025, Texas witnessed the first measles death in over a decade (Shastri and Seitz, 2025).

One reason that vaccination rates are declining is because more children in school are receiving vaccine exemptions. Most states allow parents to opt their children out of required vaccines on personal, philosophical, or religious grounds (Blank, Caplan, and Constable, 2013). The nationwide rate of vaccine exemptions among kindergartners rose from 1 percent in 1991 to

[1] Co-authored with Samantha Gold. We thank members of the Harvard Health Inequality Lab for thoughtful comments.

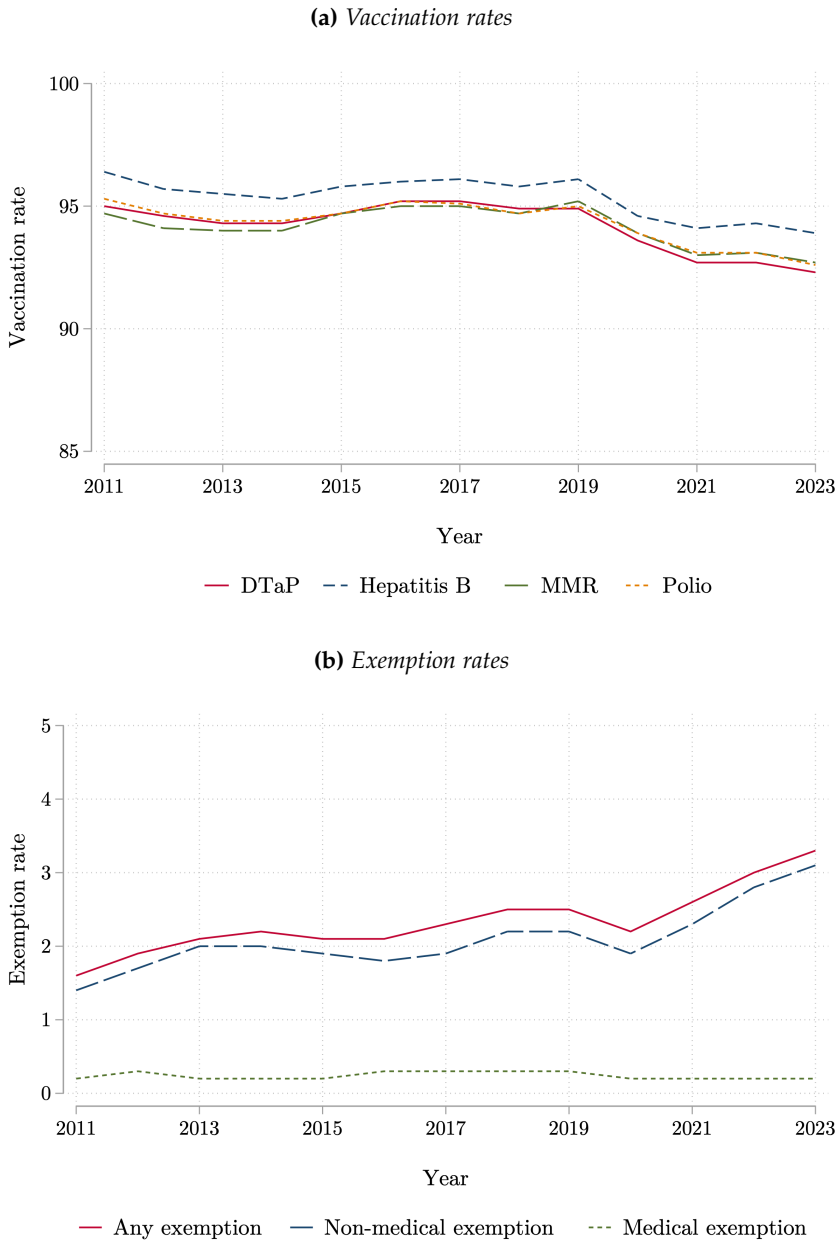
over 3 percent in 2023 (Omer et al., 2006). This trend is consistent with rising vaccine hesitancy, particularly since the Covid-19 pandemic. According to a Gallup poll conducted in 2024, 20 percent of adults think that vaccines are more dangerous than the diseases they prevent, up from 6 percent in 2001 (Jones, 2024).

In this paper, we study how state policies to enforce school vaccination requirements affect child vaccination rates. In response to rising vaccine hesitancy and several highly publicized disease outbreaks, four states repealed non-medical vaccine exemptions for schoolchildren in the last decade.[2] We use data from the CDC and an event study design to show that repeal of non-medical exemptions caused overall exemptions to fall by 4 percentage points. We observe little substitution toward medical exemptions in response to repeal. We hypothesize that state control over the issuance of medical exemptions was effective at limiting this margin of substitution.

Kindergarten vaccination rates rose within two years following non-medical exemption repeal. Results show an increase of 2 percentage points for Hepatitis B vaccination and 4 percentage points for DTaP, MMR, and Polio vaccination. We demonstrate the robustness of our results to different sample definitions, controls, and event study estimators. Effects are similar during the Covid-19 pandemic, which suggests that state exemption policy plays a critical role in keeping vaccination rates high. In the face of rising vaccine hesitancy, all four states that restricted exemptions in the past decade were above the 95 percent threshold for herd immunity in 2023.

[2] The states and respective years of repeal are California (2015), Connecticut (2021), Maine (2019), and New York (2019). See Appendix B.1 for details on state legislation.

Figure 2.1: *Trends in kindergarten vaccination and exemption rates*



Notes: Figure shows national trends in vaccination and exemption rates for children entering kindergarten from 2011 to 2023. Vaccination rates are shown for DTaP, HepB, MMR, and Polio. Exemption rates are shown as the total exemption rate and the subcategories of non-medical and medical exemptions. Data are sourced from the CDC (see Section 2.3.2 for details).

While state restrictions on vaccine exemptions can compel families to vaccinate their children, state enforcement may be necessary to ensure compliance. We use the example of the California Department of Education, which began conducting immunization audits in 2021. The goal of

the audits was to ensure that unvaccinated students were excluded from school attendance calculations, which can affect school funding. Audits were targeted to schools with greater than 10 percent vaccine non-compliance among kindergarten students. We explore the effects of auditing using the 10 percent threshold in a regression discontinuity (RD) design. We find no evidence of improved vaccine compliance in future years for schools at the audit threshold. We conclude that state enforcement of immunization and attendance rules, while important for proper allocation of state funding, does not raise itself raise vaccination rates.

This paper contributes to several strands of literature. Prior papers in economics and public health have studied the effects of changing state vaccination and exemption requirements (Omer et al., 2006; Abrevaya and Mulligan, 2011; Blank, Caplan, and Constable, 2013; Yang and Debold, 2014; Bradford and Mandich, 2015; Delamater, Leslie, and Yang, 2016, 2017; Lawler, 2017; Carpenter and Lawler, 2019; Richwine, Dor, and Moghtaderi, 2019; Bednarczyk et al., 2019; Churchill, 2021; Hair, Gruber, and Urban, 2021; Correira et al., 2024). The paper most closely related to ours is Richwine, Dor, and Moghtaderi (2019), which examines the repeal of California non-medical exemptions in 2015. Our contribution is to examine the complete set of exemption policy changes over the prior decade using data from the CDC and innovations in event study methodology (Callaway and Sant'Anna, 2021). Furthermore, we add a novel analysis of schools audits as one component of state enforcement.

We contribute to broader work on the take-up of preventative healthcare. In the case of rising vaccine hesitancy, more individuals may perceive the costs of vaccination as greater than the benefits conferred by immunity (Bauch and Bhattacharyya, 2012). Financial incentives to become vaccinated (in the context of Covid-19 vaccination) have been shown to be effective or ineffective depending on the context and design (Chang et al., 2021; Campos-Mercade et al., 2024). Vaccination recommendations can be effective in settings where new vaccines are recommended by official organizations, as was the case with the roll-out of the Hepatitis A vaccine (Lawler, 2017). Other studies find that recommendations are effective when they come from laypeople but not from healthcare experts (Ho et al., 2022; Alsan and Eichmeyer, 2024).[3] We consider the approach of several states to repeal the personal exemption entirely. Vaccination rates rise quickly following

[3] For studies on behavioral approaches to increase take-up of preventative care, see Rutter, Steadman, and Quine (2006), Loewenstein, Brennan, and Volpp (2007), Chapman et al. (2010), and Milkman et al. (2011).

repeal, in part because families have very few ways of avoiding immunization requirements post-repeal.

Finally, we contribute to a growing literature on state mandates, individual beliefs, and policy polarization (Wheaton, 2022; Alsan et al., 2023; Colgrove, 2023; DellaVigna and Kim, 2024). We show that state enforcement of school immunization requirements has been concentrated in “blue" states (defined as states that voted Democratic in the 2016 presidential election). By contrast, more conservative “red" states have acted in the opposite direction by loosening vaccine requirements. This divergence in state policy is visible in child vaccination rates. Based on our event study estimates, we show that two-thirds of the gap in vaccination rates between red and blue states is driven by state exemption policy.

This paper proceeds as follows. Section 2.2 describes the historical context and modern policy landscape for state vaccination requirements. Section 2.3 discusses the data sources. Section 2.4 provides the estimation strategy and results for the impact of non-medical exemption repeal on exemption and vaccination rates. Section 2.5 reports results for the effect of immunization audits in California on vaccine non-compliance. Section 2.6 concludes.

2.2 Background

2.2.1 Vaccine exemptions

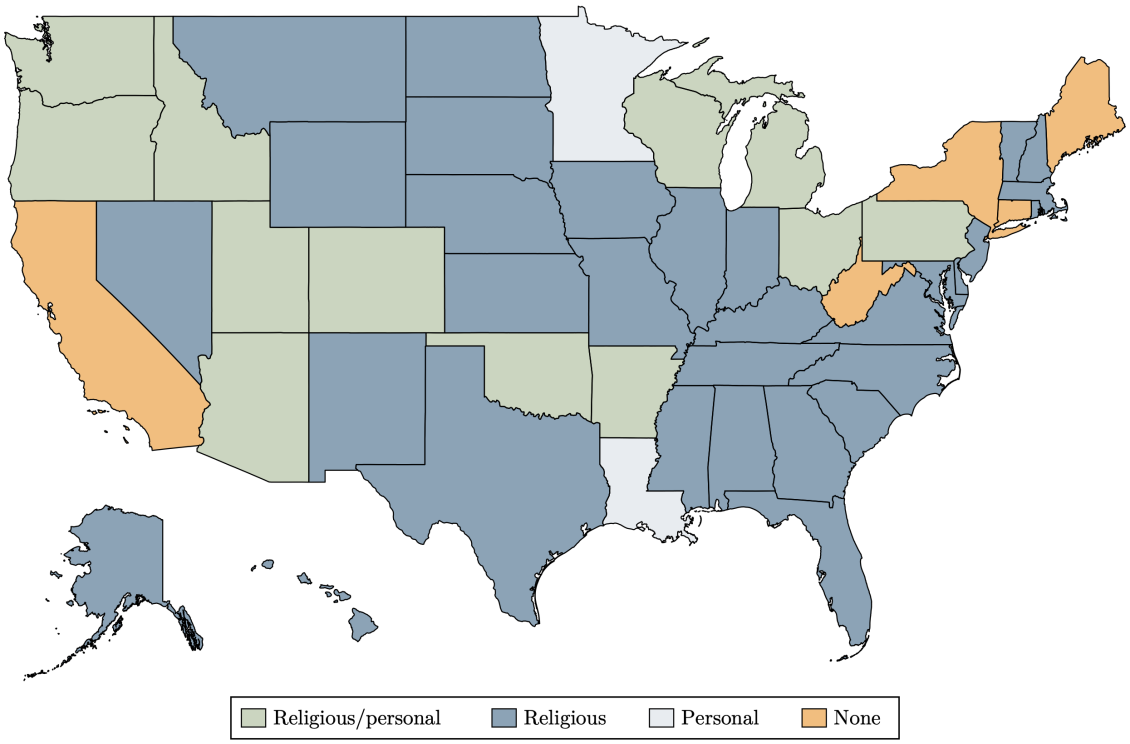
States have required children attending school to be vaccinated since the mid 19th century. The smallpox vaccine was developed in 1796, and Massachusetts began the first state to require inoculation in 1855 (Holtkamp, 2020). In the early 20th century, as secondary schooling became widely available, public health officials were concerned with disease spread in the classroom (Duffy, 1974; Meckel, 2013). During this period, more state and local governments passed vaccination mandates. Enforcement of compulsory vaccination laws was held to be within the power of states to protect public health (*Jacobson v. Massachusetts 1905*). Smallpox vaccination mandates succeeded in lowering smallpox rates and improved educational outcomes (Holtkamp, 2020). However, these efforts also generated short-lived anti-vaccination backlash (Brehm and Saavedra, 2024).

Over the course of the 20th century, new vaccines were developed and added to the required immunization schedule for schoolchildren. By 1980, all states mandated vaccination, with various exemptions in place (Omer et al., 2009). The scope of these exemptions has been a constant in health policy debates (Salmon et al., 2005; Bednarczyk et al., 2019; Colgrove, 2023). All states allow medical exemptions, which are meant for children with severe allergies or other conditions that make vaccination risky. Such exemptions must be signed by a licensed healthcare provider, typically a physician, nurse practitioner, or physician assistant. By contrast, there exists substantial variation in state policies regarding non-medical (personal, philosophical, or religious exemptions) exemptions. Non-medical exemptions might require parents to sign a form, obtain signatures from school officials, or watch an informational video. Blank, Caplan, and Constable (2013) find that in states where obtaining an exemption is more difficult, exemption rates are lower. This suggests that even minor changes to the exemption process can affect vaccination coverage.

Figure 2.2 plots the categories of non-medical exemptions allowable for children in each state as of 2024. California, Connecticut, Maine, New York, and West Virginia do not allow any non-medical exemptions. Remaining states vary in the types of exemptions they accept. It is important to note that changes to vaccine exemption policies are frequently proposed in state legislatures, even if few are signed into law. Nearly all of the enforced changes to state policy in the period that we study (2011-2023) have made exemptions harder to obtain. Prior to our study period, in 2003, Texas and Arkansas had to revise their laws to allow personal beliefs exemptions following challenges in federal court (Salmon et al., 2005; Hair, Gruber, and Urban, 2021). In 2023, Mississippi's exemption policy was challenged and religious exemptions were allowed for the first time.[4]

[4] Though 2023 was the first school year that religious exemptions were allowed in Mississippi, the number of religious exemptions surpassed the total number of medical exemptions (see here).

Figure 2.2: *Map of non-medical exemptions*



Notes: Figure shows a map of state non-medical vaccine exemptions for children entering kindergarten as of 2024. Exemption laws are sourced from LexisNexis via the National Conference of State Legislatures.

In this paper, we focus on total repeal of non-medical exemptions since 2015, beginning with California. In 2015, California experienced a substantial outbreak of measles originating in Orange County, which prompted the state to repeal non-medical exemptions (Richwine, Dor, and Moghtaderi, 2019). Since then, Connecticut, New York, and Maine have all followed suit (in 2021, 2019, and 2019, respectively).[5] We motivate our empirical strategy by showing the raw trends in non-medical exemptions in Appendix Figure B.1, Panel (a) for the 2011 to 2023 period. Following repeal, the rate of non-medical exemptions falls to zero mechanically. By plotting the trend for states that never repeal non-medical exemptions (never-treated states), we can see parallel trends in exemptions prior to repeal. In Panel (b), we show the trends for MMR vaccination.

[5] We note here several other policy changes that fell short of total repeal. Washington changed its exemption policy in 2019, when it repealed the personal exemption for MMR only. In 2015, Vermont repealed philosophical exemptions, but not religious exemptions. Since these policy changes were not total repeals, we consider Washington and Vermont as "never-treated" states in Section 2.4.2, and show that our results are robust to excluding these two states.

Relative to never-treated states, states that repealed non-medical exemptions see increases in MMR vaccination. Vaccination rates converge to that of Mississippi and West Virginia, two states that never allowed non-medical exemptions (plotted in black). The staggered repeal and changes in raw trends observed here motivate the empirical strategy in Section 2.4.

2.2.2 State audits

The long-run success of vaccination policy relies on continued enforcement of school vaccination requirements. Without enforcement, some schools might have high rates of vaccine non-compliance and remain well below the threshold for herd immunity. One possible enforcement mechanism is state-conducted audits.

The California Department of Education began conducting audits in 2021. The purpose of these audits was not necessarily to improve vaccine compliance, but to ensure schools were properly reporting student attendance. The relevant measure of attendance for school funding allocation in California is average daily attendance (ADA). ADA is calculated as the total days of student attendance divided by the total number of instruction days. Students that are not compliant with state immunization requirements must be excluded from ADA, and California conducts audits to ensure that this exclusion is taking place. The potential punishment for schools over-reporting student attendance is that school funding payments may be withheld or rescinded.[6] We hypothesize that audits may prompt schools to prevent future audits by increasing vaccination rates.

2.3 Data Sources

2.3.1 State vaccine exemption laws

We identify changes to state exemption laws using LexisNexis (via the National Conference of State Legislatures).[7] We examine each state's legislative text to verify the provisions of new laws and we report these details in Appendix B.1. We are primarily concerned with changes concerning

[6] See here for details from the California Department of Public Health.

[7] See information on exemptions provided by NCSL here.

non-medical vaccine exemptions since 2015, including complete repeal, partial repeal, or other modifications.

2.3.2 State vaccination and exemption rates

We obtain state vaccination and exemption rates from the CDC's National Center for Immunization and Respiratory Diseases (CDC 2024).[8] States report vaccination rates to the CDC for children in kindergarten, a common checkpoint that states use to enforce school immunization requirements. States differ in how they measure vaccination and exemption rates, with some states using a full kindergarten census and others using various sampling procedures. The benefit of CDC data is that it aggregates estimates from all states, allowing us to study vaccine policy changes across many states and years. From these data, we construct a panel of statewide kindergarten vaccination and exemption rates covering the 2011 to 2023 school years.[9]

The kindergarten data include coverage for the following vaccines: Diphtheria, tetanus, acellular pertussis (DTaP); Hepatitis B; Measles, mumps, rubella (MMR); Poliovirus (Polio); and Varicella. Coverage is defined by the states and indicates the percent of kindergarten students who are up-to-date according to state regulations.[10] We focus our analysis on DTaP, Hepatitis B, MMR, and Polio, excluding Varicella due to differences in how states report children with prior infection. The CDC data also contain overall exemptions rates, categorized into medical and non-medical exemptions.[11] Some states are partially missing outcome data for the 2011-2023

[8] See the CDC's webpage here.

[9] We begin the panel in 2011 because many states are missing CDC data from 2009 and data from 2010 are unavailable. The most recent school year with available CDC data is 2023. The data also contain the CDC's national estimates of vaccine coverage, which we use to produce the time series in Figure 2.1.

[10] For instance, in the 2023-24 school year, kindergarten students were considered up-to-date for DTaP if they had received 3 doses in Nebraska, 4 doses in Kentucky, Maryland, Wisconsin, or Wyoming, and 5 doses in remaining states (Seither et al., 2024).

[11] Apart from vaccination and exemption, there is a third group of students that are non-compliant (not fully vaccinated with no exemption filed). Some states record these students as "provisionally enrolled" if they are on track to complete vaccination within a set time frame (also known as a grace period). A limitation is that vaccination records are reported to the CDC at a single point in time (often in the fall, but assessment dates vary across states). It is not possible to known whether students who were non-compliant at the time of assessment completed all required vaccinations or filed an exemption.

period. To construct the main analysis sample used in Section 2.4, we restrict the data to form a balanced panel of states for each outcome from 2011 to 2023.[12]

2.3.3 State and county characteristics

We measure economic conditions using state unemployment rate, sourced from the Bureau of Labor Statistics. We measure demographics using the Surveillance, Epidemiology, and End Results (SEER) Program at the National Cancer Institute. County-level population files are used to estimate state racial composition in each year.

2.3.4 Dataset of California schools

We construct a dataset of California schools in order to study how immunization audits affect vaccine compliance. The California Department of Education maintains a directory of schools, with information on school type, grades served, and enrollment. We merge this directory to annual school-level data from the California Department of Public Health from 2010 to 2022. This merge provides rates of vaccination, exemption, and non-compliance, as well as audits starting in 2021.

To assess the impact of audits, we construct a sample of public California elementary schools with available immunization data in both 2021 and 2022 (as far as we are aware, no private schools were audited). Schools may not have available immunization data in both 2021 and 2022. The primary reason is that immunization data for schools with fewer than 20 kindergarten students is suppressed, so many small schools are dropped from the sample. By merging this sample of schools to audit records, we can observe how the first round of audits in 2021 affected vaccine compliance and likelihood of audit in 2022. We do not currently observe data beyond the 2022 school year, nor do we observe the outcome of individual audits.

[12] We allow the set of states to vary for each outcome and subsequently verify that this does not affect our results. The final number of states included in the estimation is 38 for DTaP, 37 for Hepatitis B, 39 for MMR, 39 for Polio, 43 for overall exemptions, 42 for medical exemptions, and 43 for non-medical exemptions.

2.4 Non-Medical Exemption Repeal and State Vaccination Rates

2.4.1 Estimation strategy

The staggered repeal of vaccine exemptions over time lends itself to an event study approach, where treated (repeal) states are compared to untreated (non-repeal) states before and after repeal. Define T_s^* as the first school year after the repeal of non-medical exemptions in state s. For instance, New York signed its exemption repeal on June 13th, 2019, effective for the 2019-20 school year. Maine signed its repeal in May 2019, but the repeal did not take effect until the 2021-22 school year. We assign treatment year $T_s^* = 2019$ for both New York and Maine. This allows us to see how vaccination rates respond dynamically to the passage of legislation, even before legislation takes effect. Similarly, we assign $T_s^* = 2015$ for California and $T_s^* = 2021$ for Connecticut.[13] For additional details on state legislation, see Appendix B.1.

We estimate the following event study specification:

$$y_{st} = \theta_s + \delta_t + X_{st} + \sum_{e=-5}^{-2} \pi_e D_{st}^e + \sum_{e=0}^{4} \beta_e D_{st}^e + \varepsilon_{st}, \tag{2.1}$$

where y_{st} is the vaccination or exemption rate in state s and school year t, θ_s is a vector of state fixed effects, δ_t is a vector of school year fixed effects, X_{st} is a vector of time-varying controls, and ε_{st} is an error term. The binary indicator $D_{st}^e = \mathbf{1}[t - T_s^* = e]$ is equal to 1 when state s in year t is e years away from initial treatment year T_s^*. The coefficients π_e for $e \in \{-5, -2\}$ measure whether treated states trended differently than untreated states two to five years prior to treatment. Similarly, the coefficients β_e for $e \in \{0, 4\}$ measure the treatment effect e years after treatment. The π_e and β_e coefficients are relative to $e = -1$. We estimate equation (2.1) with and without controls for state unemployment rate, percent Hispanic, and percent Black. For inference, we report standard errors clustered at the state level.

[13] To obtain unbiased estimates, difference-in-differences requires a "no anticipation" assumption (Baker et al., 2025). In our setting, this requires that families did not change vaccination behavior prior to the passage of the law. We find this assumption plausible given the short time frame of vaccine legislation. California's bill was introduced in February of 2015 and signed in June, leaving no scope for families to respond prior to the 2015 school year (which we code as the first post-treatment year). Likewise, Connecticut's bill was introduced in February of 2021 and signed in April, Maine's bill was introduced in February of 2019 and signed in May, and New York's bill was introduced in January of 2019 and signed in June.

Recent literature has emphasized several concerns with the event study approach in equation (2.1), also known as a dynamic two-way fixed effects (TWFE) specification. Much of the concern revolves around how to construct a relevant comparison group for treated units, how to interpret estimates in the presence of dynamic and/or heterogeneous treatment effects, and how to test for parallel trends.[14] Our approach is to estimate equation (2.1) using the methodology of Callaway and Sant'Anna (2021). We report results using never and not-yet treated states as controls and show the sensitivity of our estimates to alternative approaches. In addition, rather than reporting a single TWFE estimate that aggregates across periods, we prefer to report the full set of dynamic effects.

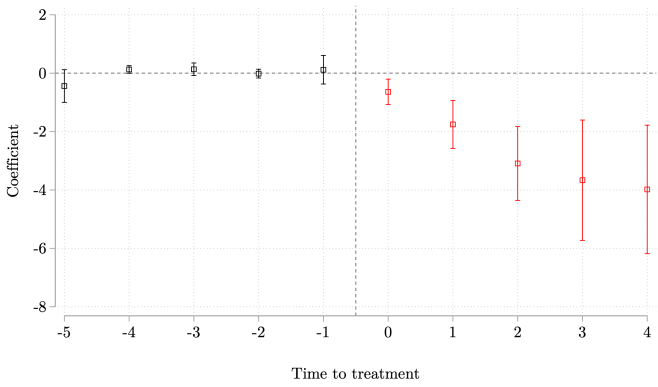
2.4.2 Results: Exemption rates

We start by plotting how exemption rates respond to legislation. Figure 2.3, Panel (a) plots estimates of β_e, the effect of state repeal on the rate of total vaccine exemptions in each post-repeal school year e (in red). The removal of non-medical exemptions caused the rate of exemptions among kindergartners to drop by 4 percentage points (statistically significant at the 95-percent confidence level). Pre-treatment coefficients π_e show no evidence of a differential pre-trend in treated states prior to repeal (in black). Since non-medical exemptions make up the majority of exemptions, this drop is largely mechanical. As shown in Figure 2.3, Panel (b), the decline in non-medical exemptions mirrors the decline in total exemptions. In Appendix Figure B.2, we report event study estimates by group following Callaway and Sant'Anna (2021). In our setting, groups are defined as states treated in different years. We report results for states treated in 2015 (California), 2019 (Maine and New York), and 2021 (Connecticut). The effect of exemption repeal is similar across groups despite differences in treatment timing and state characteristics.

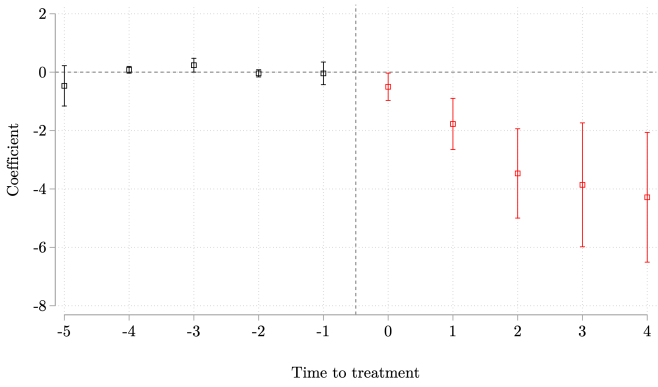
[14] See, for instance, de Chaisemartin and D'Haultfœuille (2020), Goodman-Bacon (2021), Sun and Abraham (2021), Roth (2022), and Borusyak, Jaravel, and Spiess (2024).

Figure 2.3: *Exemption repeal and state exemption rates*

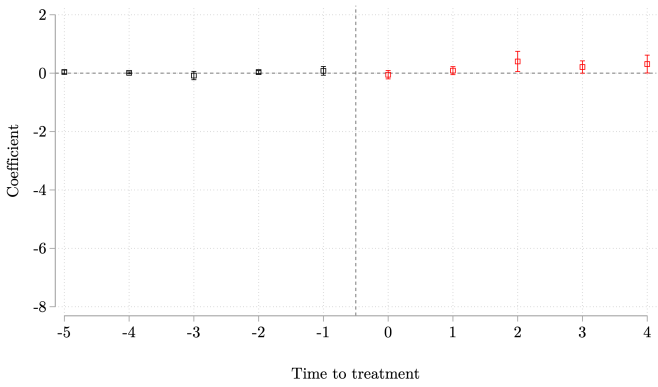
(a) *All exemptions*



(b) *Non-medical exemptions*



(c) *Medical exemptions*



Notes: Figure shows event study coefficients from equation (2.1). Each panel plots the π_e coefficients on the pre-treatment years in black and the β_e coefficients on the post-treatment years in red. Coefficients are estimated using the event study methodology of Callaway and Sant'Anna (2021). 95 percent confidence intervals are shown with standard errors clustered at the state level. The dependent variable in Panel (a) is the rate of children entering kindergarten receiving any vaccine exemption. Results for the two subcategories of exemptions, non-medical and medical, are shown in Panels (b) and (c). Data are sourced from the CDC (see Section 2.3.2 for details).

In some states, repeals were not immediately enforced. For instance, in Maine, non-medical exemptions were repealed in 2019 ($e = 0$ in our specification) but the law did not take effect until 2021 ($e = 2$). This explains the increasing magnitude of the exemption coefficients observed in Figure 2.3, Panels (a) and (b). We can see further evidence of these dynamics in Appendix Figure B.2. Panel (a) shows that non-medical exemptions didn't fall until 2016 when California's law took effect. Similarly, Maine's 2019 law didn't take effect until 2021, which explains the increase in magnitude of treatment effects observed in that year in Panel (c). In 2021 (Panel (e)), Connecticut's law took effect immediately, but allowed families flexibility when complying with state requirements.

Medical exemptions are still allowable in all states as of 2024. As discussed in Richwine, Dor, and Moghtaderi (2019), if medical and non-medical exemptions are easily substituted, we might observe a rise in medical exemptions once non-medical exemptions are repealed. We examine this hypothesis in Figure 2.3, Panel (c). Following the repeal of non-medical exemptions, we observe some degree of substitution toward medical exemptions. Several coefficients are positive and statistically significant post-repeal, however, the magnitudes are small (less than 0.5 percentage points). In Appendix Figure B.2, we see that these effects are driven by California's 2015 repeal (Panel (a)). Notably, at the start of 2021, California ruled that all medical exemptions must be issued and reviewed via the centralized California Immunization Registry (CAIR). In the 2021 school year, the coefficient on medical exemptions is no longer statistically significant.

To summarize the event study results, Table B.1 reports coefficients from equation (2.1) for overall, non-medical, and medical exemption rates (columns (1)-(3)). We observe insignificant pretrends in exemptions prior to state law passage, followed by a drop in non-medical exemptions as the laws take effect. Relative to the mean exemption rate in treated states, repeal of non-medical exemptions causes the overall exemption rate to fall to nearly zero. The slight uptick in medical exemptions is statistically significant and large relative to baseline. However, as shown in Appendix Figure B.2, this effect is driven by California and does not persist after six years.

We assess the robustness of the event study results in several ways. We first examine robustness to different event study estimators. In Appendix Figure B.3, we show that our results are similar using a variety of different approaches developed in Sun and Abraham (2021), Borusyak, Jaravel, and Spiess (2024), and de Chaisemartin and D'Haultfœuille (2020). We also

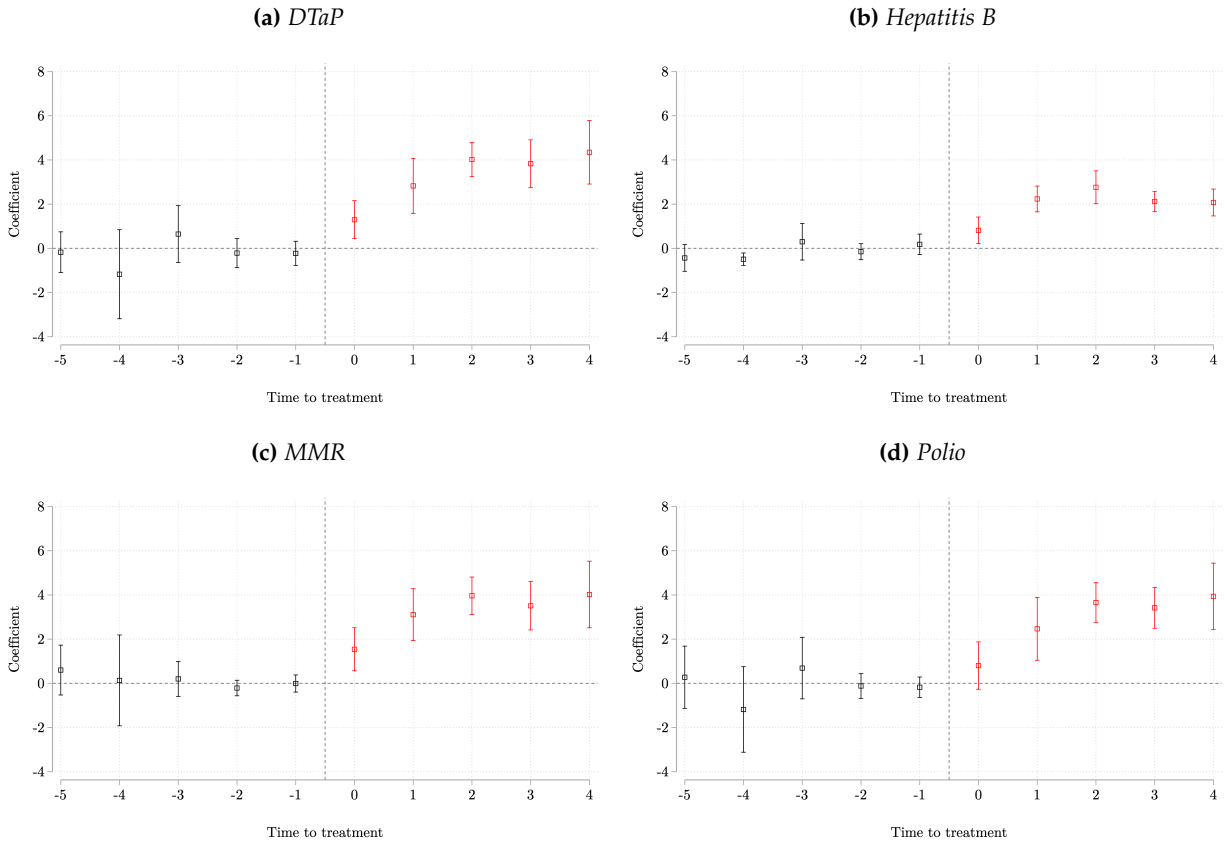
show that (potentially biased) estimates from a standard TWFE event study are highly similar to our main results. This suggests that the potential issues with staggered treatment adoption are not substantial in our setting. In Appendix Figure B.4, we explore whether our main event study estimates are sensitive to the weighting scheme, sample definition, or controls used. Adding population weights slightly shrinks the magnitude of the treatment effects. Similarly, when we restrict the sample to a uniform set of states across all outcomes, the relative decline in exemptions is smaller. This is consistent with the presence of several small states in the control group that report high exemption rates and are dropped from the uniform sample. The addition of time-varying controls does not change the estimates. Finally, we restrict the set of comparison states in multiple ways. We estimate equation (2.1) and only include “never-treated" states. We drop partially treated states (Washington and Vermont), which restricted exemptions during the sample period but stopped short of a full repeal (see Appendix B.1). As a final check, we note that all treated states consistently align with the Democratic party in presidential elections. We restrict to Democrat-leaning states (defined as states that voted for Hillary Clinton in the 2016 presidential election). In all of these robustness checks, we find event study estimates that are similar to our main results in terms of magnitude and statistical significance.

2.4.3 Results: Vaccination rates

Figure 2.4 plots estimates of β_e for several vaccination outcomes. Results in Panel (a) show that following the repeal of non-medical exemptions, DTaP vaccination among kindergartners increased by 4 percentage points relative to comparison states. We observe similar effects for Hepatitis B, MMR, and Polio vaccination (Panels (b)-(d), respectively).[15] Note that while the effect size for Hepatitis B is smaller, the baseline vaccination rate against Hepatitis B is highest among the vaccines that we consider (Figure 2.1). We account for the fact that the state of Washington repealed exemptions for MMR in 2019. For MMR only, we add Washington to the set of treated states to ensure that we capture the effects of this reform. Similar to the exemption results, we observe no pre-treatment trend in vaccination rates. Event study coefficients are statistically significant at the 95 percent level for all four vaccines.

[15] Maine does not require vaccination for Hepatitis B, so we drop Maine from the Hepatitis B sample.

Figure 2.4: *Exemption repeal and state vaccination rates*



Notes: Figure shows event study coefficients from equation (2.1). Each panel plots the π_e coefficients on the pre-treatment years in black and the β_e coefficients on the post-treatment years in red. Coefficients are estimated using the event study methodology of Callaway and Sant'Anna (2021). 95 percent confidence intervals are shown with standard errors clustered at the state level. The dependent variable in Panel (a) is the DTaP vaccination rate for children entering kindergarten. Results for Hepatitis B, MMR, and Polio are shown in Panels (b), (c), and (d). Data are sourced from the CDC (see Section 2.3.2 for details).

We conduct the same set of extensions as in Section 2.4.2. We verify that effects look similar by treatment year (Appendix Figure B.2, Panels (b), (d), and (f)). In California, the anticipation of state enforcement in 2016 raised vaccination rates in the 2015 school year by 1 to 2 percentage points. Similar to the exemption results, the coefficients increase in size as state enforce their exemption repeals. To summarize these results, we report coefficients from equation (2.1) in Table B.1. We observe insignificant pretrends in vaccinations prior to state law passage, followed by an increase in vaccinations as the laws take effect. Note that we do not observe vaccine-specific exemptions in the CDC data. However, the results imply that most children who were receiving non-medical exemptions in the pre-period were exempt from all vaccines. Exemption repeal

therefore increased vaccination rates similarly across the different vaccines. Finally, we show in Appendix Figures B.5 and B.6 that the vaccination results are robust to changing the event study estimator, sample definition, weighting scheme, and controls used.

Our results show that state repeal of non-medical vaccine exemptions has significantly raised vaccination rates among kindergartners.[16] However, in several cases, legislation was passed in response to notable outbreaks of measles (*e.g.*, in California and New York). Oster (2018) and Schaller, Schulkind, and Maghakian Shapiro (2017) show that parental vaccination behavior responds to broader incidence of infectious disease. This behavioral response might be driving a portion of our results. We note that Connecticut did not experience an infectious disease outbreak prior to repeal, and effects for Connecticut are similar to other treated states (Appendix Figure B.2, Panels (e) and (f)). Oster (2018) also shows that parents respond most to outbreaks when their children are very young or about to enter kindergarten. Rather than the observed effects becoming smaller as memory of the outbreak fades, the effects grow and persist for many years. Finally, we might expect the largest behavioral response from parents to be measles vaccination, yet we observe statistically similar effects for DTaP and polio vaccination. In summary, we do not find evidence that suggests our results are driven by parental response to disease outbreak.

We also look for simultaneous policy changes that would complicate the interpretation of our results (Hoehn-Velasco et al., 2024). Mandates for Tdap vaccination in middle school were mostly passed prior to 2015 (Carpenter and Lawler, 2019). We find it unlikely that mandates for older children would influence vaccination of children entering kindergarten. Hepatitis A mandates for young children were also passed in many states prior to 2015, and Lawler (2017) finds no evidence of spillover effects on take-up of other vaccines. Importantly, our sample period overlaps with the Covid-19 pandemic and corresponding state Covid-19 policies, which can pose a challenge for difference-in-differences approaches in general (Goodman-Bacon and Marcus, 2020).

[16] So far, we have focused on students entering kindergarten as the population of interest. In analysis not shown, we test whether state repeal of non-medical exemptions increased vaccination rates for older children. We find no evidence that DTaP, Hepatitis B, MMR, or Polio vaccination rates rose among children ages 13-17. This is likely due to several factors. First, children with pre-existing non-medical exemptions were typically allowed to keep them (see Appendix Section B.1 for details by state). This means that most older children were unaffected by the law changes, and only children newly enrolled in kindergarten are likely to show increased vaccination rates. Second, the vaccination rates for older children come from the National Immunization Surveys (NIS-Teen). See the CDC's TeenVaxView here. These data are generated from phone surveys, so estimates of teen vaccination rates are far noisier than the measured vaccination rates for students in kindergarten. These data also do not contain information on school vaccine exemptions, which limits our ability to understand outcomes for this age group.

In our context, routine child vaccinations fell during the pandemic as a direct result of pandemic restrictions and the shift to remote instruction (DeSilva et al., 2022). It is possible that the states that repealed exemptions in 2019 would have experienced even larger treatment effects absent the Covid-19 pandemic (see Appendix Figure B.2, Panel (d)). Given that we observe similar treatment effects for California's exemption repeal in 2015, we emphasize that our results generalize beyond the pandemic period.

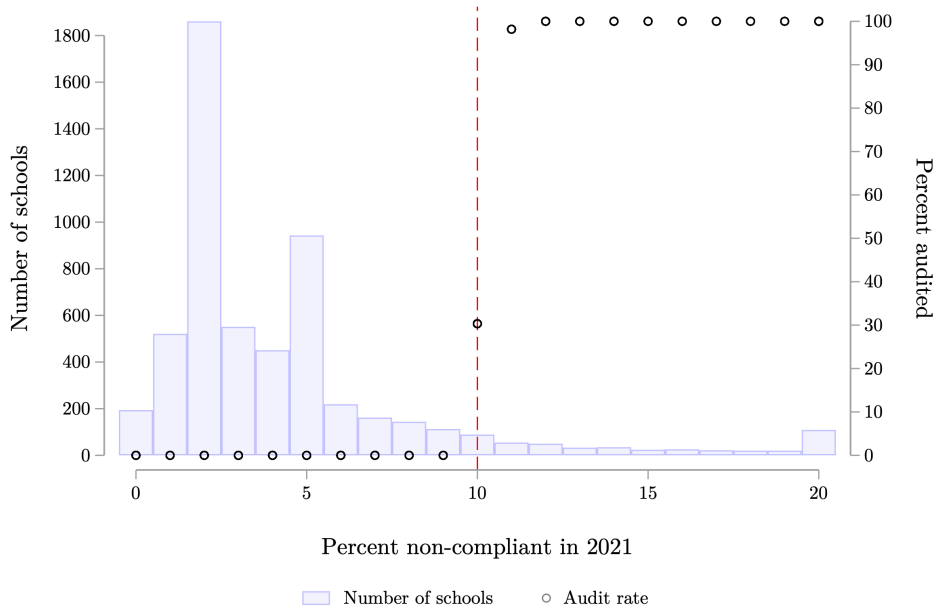
Given the polarization in state policy between politically conservative and liberal states (DellaVigna and Kim, 2024), we examine the divergence in child vaccination rates. Define "blue" states as states that voted Democratic in the 2016 presidential election (and likewise, "red" states as states that voted Republican). Figure B.7 shows the trends in population-weighted DTaP vaccination rates between blue and red states. A gap emerged in 2015 and increased to roughly 2 percentage points by 2023. We use the group-specific coefficients shown in Figure B.2 to estimate the vaccination rate that would have prevailed had blue states not repealed non-medical exemptions. This counterfactual trend is plotted in black. The shaded area indicates that by 2023, two-thirds of the gap in DTaP vaccination between red and blue states can be attributed to state exemption policy.

2.5 State Enforcement via Audits

In this section, we explore whether school audits in California since 2021 have increased vaccine compliance beyond the effects of the legislation passed in 2015. We use the structure of audits to identify whether schools that were audited exhibit higher vaccine compliance in future years. The state audited schools with a non-compliance rate greater than 10 percent in 2021. Figure 2.5 shows the sharp discontinuity in auditing for schools at the 10 percent threshold. The distribution of non-compliance rates is smooth around the threshold, which lies in the upper tail of the distribution. Given that 2021 was the first year of audits, there was little scope for schools to anticipate the audit threshold. Rounding in the public data causes some unaudited schools to appear at or above the threshold. Rather than use a fuzzy regression discontinuity design, we reassign the unaudited schools above the 10 percent audit threshold to just below the threshold.

This is equivalent to assuming that California correctly audited all schools and that a sharp regression discontinuity design is appropriate in this setting.

Figure 2.5: *Non-compliance rates in 2021*



Notes: Figure shows a histogram of vaccine non-compliance rates in California schools in 2021. Left y-axis shows the number of schools. Right y-axis shows the percent of schools in each bin that were audited in 2021. See Section 2.5 for details.

Specifically, we compare schools that were audited (non-compliance greater than 10 percent) with schools that were just below the audit threshold (non-compliance of 10 percent or less):

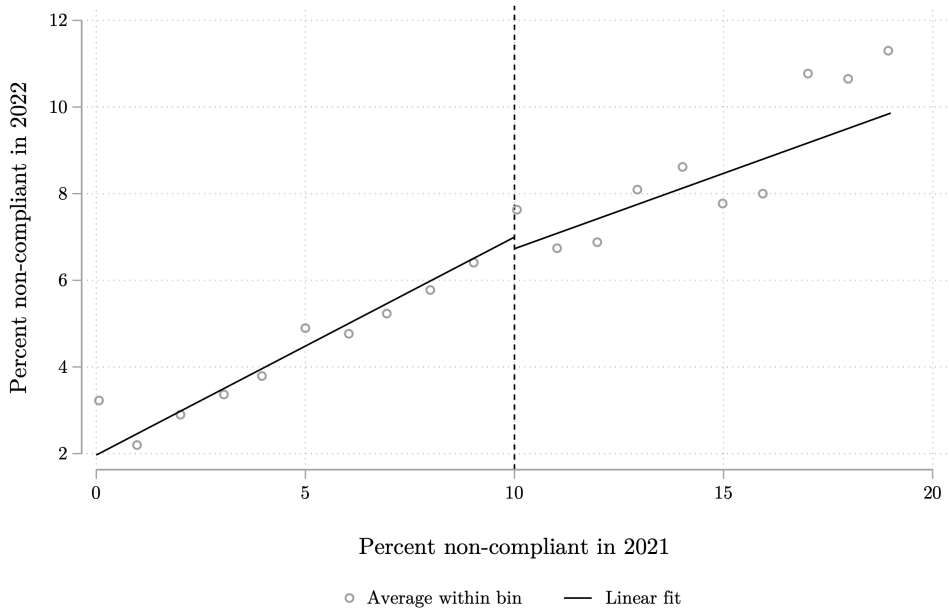
$$y_s = \gamma_0 + \gamma_1 \mathbf{1}(NC_s > 10) + \gamma_2(NC_s - 10) + \gamma_3 \mathbf{1}(NC_s > 10)(NC_s - 10) + \nu_s, \tag{2.2}$$

where y_s is the outcome for school s in 2022, $\mathbf{1}(NC_s > 10)$ is an indicator for whether school s was above the audit threshold of 10 percent vaccine non-compliance in 2021, and $(NC_s - 10)$ is the distance of school s from the audit threshold. We include the interaction term $\mathbf{1}(NC_s > 10)(NC_s - 10)$ to allow for a linear relationship between the running variable and the outcome that varies on either side of the threshold.

Figure 2.6 plots the running variable (non-compliance in 2021), the binned outcome (non-compliance in 2022), and a linear fit above and below the audit threshold. The outcome shows no discontinuity in 2022, which suggests that the audits did not raise vaccine compliance. We test

several other outcomes, including likelihood of audit and vaccination rates in 2022. To construct point estimates, we implement equation (2.2) using optimal bandwidth selection with a triangular kernel (Calonico, Cattaneo, and Titiunik, 2014; Calonico et al., 2017). These results are reported in Table B.2. We do not find evidence of a significant difference at the audit threshold for any outcome.

Figure 2.6: *Percent of vaccine non-compliance in 2022 at the 2021 audit threshold*



Notes: Figures shows the relationship between vaccine non-compliance in 2021 (x-axis) and 2022 (y-axis). Schools to the right of the audit threshold (10 percent non-compliance) were audited. See Section 2.5 for details.

While these results show that immunization audits in California did not raise future vaccine compliance, we do not conclude that the audits were ineffective. The goal of the audits was to enforce proper school enrollment reporting for unvaccinated students. Furthermore, we note several reasons why we might not identify statistically significant effects. Our causal evidence is limited to RD estimates at the audit threshold in 2022, which leaves us with very little effective sample size. It remains a possibility that the threat of audit shifted the entire distribution of schools toward improved compliance. We also note that even if schools try to avoid a future audit, they may have little scope to improve vaccine compliance for incoming kindergarten students.

2.6 Conclusion

The decline of child vaccination rates has the potential to cause an uptick in cases and deaths from preventable infectious diseases. In this paper, we examine state actions that enforce school vaccination requirements, including the removal of non-medical exemptions and school audits in the state of California. We find robust evidence that the repeal of non-medical vaccine exemptions increased vaccination rates by 2 to 4 percentage points. This effect is sizable enough to keep overall vaccination rates above the commonly cited 95 percent immunity threshold in repeal states. The addition of school immunization audits in California in 2021 did not further raise vaccine compliance.

We highlight several limitations and avenues for future work. Our primary estimates are based on CDC data, which limits our ability to speak to dimensions like disparities in vaccination by race and socioeconomic status. Our audit results are based on only one state, and state efforts that more directly target vaccine non-compliance might be more effective at increasing vaccination rates. We focus primarily on the direct effect of state policy action, however, backlash against legislation might also occur.[17] It is an open question whether families are responding to changing state education and health policy by moving states or choosing to homeschool their children. In the face of rising vaccine hesitancy, state compulsion to vaccinate might strengthen the messaging of anti-vaccination groups. We leave these questions for future research.

[17] In the context of the Covid-19 pandemic, several studies examine the (possibly unanticipated) costs to vaccine mandates (Bardosh et al., 2022; Colgrove, 2023; Ferranna et al., 2023; Abouk et al., 2024; Gandhi et al., 2024).

Chapter 3

Economics of Foster Care[1]

3.1 Introduction

When authorities believe children have been abused or neglected, child protective services administered at the state or local level may remove those children from their homes and place them temporarily into substitute care. This practice, commonly known as foster care placement, involves a difficult tradeoff: protecting children versus preserving families. This tension has a long history in the US child welfare system. At the turn of the last century, "orphan trains" transported thousands of neglected children from cities to live and work on farms. Largely in response, early cash welfare programs, such as Mothers' Pensions and Aid to Dependent Children, were implemented with the expressed intent of keeping children with their biological families (Aizer et al., 2016; Testa and Kelly, 2020). The foster care system has changed over time, but the core debate over protecting children and preserving families remains. Today, foster care placement is common around the world: 5 percent of US children are placed in foster care at some point

[1] Co-authored with Joseph J. Doyle Jr., Max Gross, and Brian A. Jacob. Copyright *American Economic Association*; reproduced with permission. Originally published as Bald, Doyle, et al. (2022), available here: https://pubs.aeaweb.org/doi/10.1257/jep.36.2.223. We are grateful to Lawrence Berger, Eric Chyn, Kim Dadisman, Erik Hurst, Barrett Johnson, Bridgette Lery, Margarita Machelett, Enrico Moretti, Nina Pavcnik, Heidi Williams, and Fred Wulczyn for helpful comments, and especially to Timothy Taylor for extensive comments and edits. We thank Sam Haas for excellent research assistance. The data used in this publication, AFCARS Foster Care Files (1995–2019), were obtained from the National Data Archive on Child Abuse and Neglect and have been used in accordance with its Terms of Use Agreement license. The Administration on Children, Youth, and Families, the Children's Bureau, the original data set collection personnel or funding source, NDACAN, Cornell University and their agents or employees bear no responsibility for the analyses or interpretations presented here.

during childhood, and similar rates are found in other countries (Rouland and Vaithianathan, 2018; Yi, Edwards, and Wildeman, 2020).

Children who spend time in foster care are particularly vulnerable. In comparison to similar children who did not spend time in foster care, foster children have rates of depression and anxiety that are seven times higher (Turney and Wildeman, 2016) and also exhibit worse educational outcomes, such as lower test scores and graduation rates (Barrat and Berliner, 2013). These disadvantages continue into adulthood. For example, close to 20 percent of the US prison population were in foster care as youth (US Bureau of Justice Statistics, 2021). Those who turn 18 in foster care report 50 percent lower earnings and employment rates that are 20 percentage points lower by age 26 compared to a sample of young adults with similar levels of education (Okpych and Courtney, 2014); as many as one-third experience homelessness (Dworsky, Napolitano, and Courtney, 2013).

Economic theory can clarify the determinants of the demand and supply of foster care, including incentives to provide high-quality care. Econometric analysis can estimate the causal effects of foster care and related interventions on child wellbeing, and there is a growing body of empirical work on the topic. Federal and state policies now encourage the evaluation of foster care interventions and subsidize the implementation of evidence-based programs. These policies, along with increased data availability and an openness to testing new digital tools, have heightened the demand for rigorous research in the field.

In this paper, we provide an overview of the US foster care system and highlight key considerations in child welfare policy. We begin by discussing how the system works and show several prominent trends in foster care placement. We also highlight that states operate their child welfare systems quite differently, and the conditions states face and approaches they adopt have changed over time. Given this heterogeneity, we look within states and ask whether foster care placement improves outcomes for maltreated children.

We then turn to factors influencing the demand and supply of foster care. On the demand side, we discuss recent drivers of foster care, as well as prevention efforts through in-home family services and diversion from formal placement to less formal living arrangements with relatives. On the supply side, we describe the way the supervision of foster care is organized, efforts to

increase the recruitment of foster homes, and the quality of care provided by different forms of foster care placement. We conclude by highlighting opportunities for future research.

3.2 The Landscape of Child Protection

Modern child welfare programs in the United States began with the establishment of the Children's Bureau in 1912, followed by federal funding in the Social Security Act in 1935. Indeed, Title IV of the Social Security Act is titled "Grants to States for Aid and Services to Needy Families with Children and for Child-Welfare Services." A series of federal and state laws addressing child maltreatment have been enacted since, and the competing goals of family preservation and child protection can be seen through the combination of federal funding of foster care placements along with cash welfare and family unification initiatives (Haskins, 2020). Child welfare agencies currently spend over $30 billion each year on child protection, with approximately half of their funding provided by the federal government. Most recently, the Family First Prevention Services Act of 2019—commonly referred to as Family First—for the first time allows states to use federal funding on services designed to prevent foster care placement.

The path of a child into foster care most often starts with a phone call to report suspected child maltreatment. States typically classify child maltreatment into three categories: neglect, physical abuse, and sexual abuse. *Neglect* is the inability of caregivers to supply a child with adequate housing, food, clothing, or other basic needs; *physical abuse* is "any nonaccidental physical injury to the child"; and *sexual abuse* involves direct sexual contact or exploitation of children (Child Welfare Information Gateway, 2019b). Roughly 54 percent of initial referrals are deemed to warrant further investigation. The remaining referrals are screened out because they do not fall under the state-specific definition of maltreatment, although such definitions often leave room for interpretation (Font and Maguire-Jack, 2020).

People in certain occupations, such as educators, police officers, physicians, and social workers, are mandated by law to report suspected maltreatment to local authorities, though reporting requirements vary by state. The majority of screenedin reports come from mandated reporters. Educators make up 21 percent of reports, as they are frequently in close proximity with school-aged children and can observe signs of maltreatment. Law enforcement personnel are

second at 19 percent, as they may request support from child protective services when responding to domestic disputes or reports of unsupervised children. Medical personnel and social workers are also common reporters (at 11 and 10 percent of reports respectively), as they may observe injuries that raise suspicion or signs of neglect in the home. The remainder of reports come from relatives, friends, and other miscellaneous sources (US Department of Health and Human Services, 2021).

3.2.1 The prevalence of child maltreatment and foster care placement

Table 3.1: *Foster care trends*

	(1) Number in 2019	(2) Share in 2004	(3) Share in 2019	(4) Difference (3) - (2)
Panel A: Children ages 0-17 (N = 73,039,150)				
Investigated for maltreatment	3,449,674	3.52	4.72	1.19
Confirmed as victims	652,253	0.97	0.89	–0.08
Entered foster care	250,311	0.41	0.34	–0.06
In foster care at end of fiscal year	419,760	0.68	0.57	–0.11
Panel B: Removal reason for children entering care (N = 250,311)				
Removed due to neglect		51.43	63.87	12.44
Removed due to parent substance use		23.34	38.15	14.81
Removed due to physical abuse		16.88	12.94	–3.94
Removed due to sexual abuse		6.33	3.96	–2.37
Panel C: Placement setting for children at end of year (N = 418,654)				
Unrelated foster family		47.40	46.62	–0.77
Kinship care		22.61	31.94	9.33
Congregate care		18.05	10.56	–7.49
Other setting		11.95	10.88	–1.07
Panel D: Exit reason for children leaving care (N = 241,796)				
Reunification with parents		54.20	48.32	–5.88
Adoption		18.90	26.63	7.72
Transition to relatives or guardianship		16.91	17.20	0.29
Emancipation		5.95	6.40	0.45

Notes: Reported sample sizes are for 2019. Panel A reports statistics among children ages 0–17. Panel B reports the percentage of children removed for each reason among children entering foster care. Children can be removed for multiple reasons, which are not mutually exclusive. Panel C reports the most recently observed placement setting for children in care at the end of the fiscal year. This sample size is smaller than the total reported in panel A due to missing data on placement setting. Panel D reports exit reasons for children leaving care. Exits from care are mutually exclusive, where the omitted category (1.45 percent) includes agency transfers, runaways, and child deaths. See text for sources.

In 2019, child protective services investigated nearly 3.5 million children for maltreatment (nearly 5 percent of all children), and classified 652,253 children as victims or nearly 1 percent of all children, as shown in Table 3.1.[2] Yearly incidence of maltreatment reported to child protective services dramatically understates the cumulative rate of maltreatment experienced by children. Over the course of childhood, a remarkable 37 percent of all children experience a child welfare investigation, and 12 percent of children are identified as maltreatment victims, including 18 percent of Black children and 16 percent of Native American children (Kim et al., 2017; Yi, Edwards, and Wildeman, 2020).[3] These numbers only capture child maltreatment reported to authorities; self-reported victimization rates in survey data reach as high as 38 percent (Finkelhor et al., 2015).

Children identified as maltreatment victims typically remain with their family. Many of these children's families are provided with services, such as family therapy, parenting programs, or substance abuse treatment. The remaining maltreatment victims are removed from their families and placed in foster care.

Table 3.1 shows that there were over 400,000 children in foster care at the end of 2019, or 0.6 percent of the child population—down from 0.7 percent in 2004. The share of children in care has fluctuated over time, rising from 300,000 in the 1980s to over 550,000 by the early 2000s (Testa and Kelly, 2020). Trends in child maltreatment and foster care placement were somewhat more stable from 2004–2019. Panel B of Table 3.1 shows that the allegations associated with child removals have shifted substantially toward neglect (64 percent of children in 2019) and away from abuse (17 percent of children in 2019). Moreover, Wulczyn (2020) documents demographic shifts in the types of children who are entering foster care, with greater proportions entering as infants and smaller shares from large urban centers since 2000. As we explain discuss below, there are several potential explanations for this shift, including the opioid epidemic.

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[2] To characterize foster care practice across states and over time in this paper, we use data from the Adoption and Foster Care Analysis and Reporting System (AFCARS): https://www.acf.hhs.gov/cb/ data-research/adoption-fostercare/ (US Department of Health and Human Services, 2020a). For child maltreatment statistics, we use the National Child Abuse and Neglect Data System (NCANDS), a centralized system for states to submit maltreatment reports to the federal government: https://www.acf.hhs. gov/cb/research-data-technology/reporting-systems/ncands/. In particular, we use NCANDS data that have been processed and combined with population data from the US Census by the Annie E. Casey Foundation (AECF): https://datacenter.kidscount.org/ (Annie E. Casey Foundation, 2022).

[3] While the lack of comparable data makes it difficult to compare child maltreatment across countries, some evidence suggests that the United States ranks toward the top of more objective indicators of child maltreatment, such as child fatality due to intentional injury (OECD, 2013, Chart PF1.9.A).

3.2.2 Placement types and time spent in foster care

Children who are removed from home can be placed in several different types of foster care. Placement with an unrelated foster family is most common, comprising about half of children in foster care at the end of 2019, as shown in Panel C of Table 3.1. One of the major developments in foster care over the past few decades is the increased use of kinship care, a practice whereby children are placed with a nearby relative, such as a grandparent. In Table 3.1, the share of foster children in kinship care was 32 percent at the end of 2019, an increase of 9 percentage points since 2004. Just over 10 percent of foster children were placed in congregate care settings such as group homes and institutions, a rate that typically varies from 5 to 20 percent across states. As evidence has mounted showing poor outcomes for children placed in congregate care, its use has fallen out of favor (Lee et al., 2011). For instance, under the 2019 Family First legislation, federal funding only covers the first two weeks of placement into congregate care.

The typical goal for a foster child is a return home, although for some the goal is placement in an adoptive home. In 2019, 48 percent of all children who exited foster care were reunified with their families, a 6 percentage-point decrease from 2004 (Panel D of Table 3.1). The next most common exit types are adoption (27 percent), living with relatives or guardians (17 percent), and emancipation as a legal adult (6 percent).[4] Adoption in particular has become more common in recent years, following legislation in the 1990s to incentivize shorter stays (Haskins, 2020).

In terms of the foster care experience, among children who entered foster care in 2015, the median length of stay was 15 months, although some children stay much longer. Ten percent of children entering in 2015 were still in foster care four years later, but in three states this fraction exceeded 20 percent. With longer stays often come multiple foster homes, and placement stability is often considered a quality metric for child welfare systems due to its strong correlation with behavioral outcomes (Rubin et al., 2004). At the end of the 2019 fiscal year, 35 percent of children in foster care had lived in three or more different settings.

[4] The AFCARS data used in Table 3.1 is not the ideal data set for measuring high frequency entry and exit dynamics. The underlying state-level data is collected in semi-annual reporting windows, and states report information from a child's most recent removal event in a given window.

3.2.3 Racial disparities in the child welfare system

Racial disparities exist at every decision point in the child welfare system, resulting in large differences in involvement with child protective services by the time children reach age 18. For instance, Black children are reported to child protective services more frequently than White children, and reports involving Black children are more likely to be confirmed as maltreatment (Putnam-Hornstein et al., 2013). Furthermore, an estimated 10–12 percent of Black children and 11–15 percent of American Indian/Alaska Native children will experience a foster care placement over the course of their childhood, compared to 5 percent of White children (Wildeman and Waldfogel, 2014). Understanding the causes and consequences of disparities, and what policies can be enacted to reduce them, is an active area of research (for overviews, see Barth et al. (2020) and Dettlaff and Boyd (2020)).

3.2.4 Heterogeneity across child welfare systems

Child welfare systems are administered at the state or local level; therefore the emphasis on child protection versus family preservation can fluctuate over time and across jurisdictions. For example, there are prominent examples of a community experiencing a tragedy, such as a case of severe abuse or a child fatality, that led to policy changes emphasizing child protection followed by a steep rise in placement rates. Years later, the higher cost of maintaining a larger share of children in foster care may be met with reforms to speed reunification and reduce placement rates through family preservation programs.[5]

Partially as a result of local policies, foster care placement rates vary substantially across states. Figure 3.1, Panel (a) shows the distribution of foster care placement rates across states in 2019, which range from under 3 per 1,000 children in New Jersey, Utah, and Virginia to over 15 per 1,000 children in West Virginia, Montana, and Alaska. Even within states, there have been considerable changes over time. Between 2004 and 2019, the rate per 1,000 children in foster care nearly doubled in West Virginia (10 to 20.2) and Montana (9.1 to 16.1), but fell substantially in Nebraska (14 to 7.1) and the District of Columbia (21.6 to 5.2). Such heterogeneity may also

[5] The State Child Abuse & Neglect Policies Database (https://www.scanpoliciesdatabase.com/), which provides detailed state-level information on maltreatment definition and policy context, is a useful resource to understand the varying policy landscapes.

reflect demand-side factors, although they are not well explained by differences in official rates of maltreatment.[6]

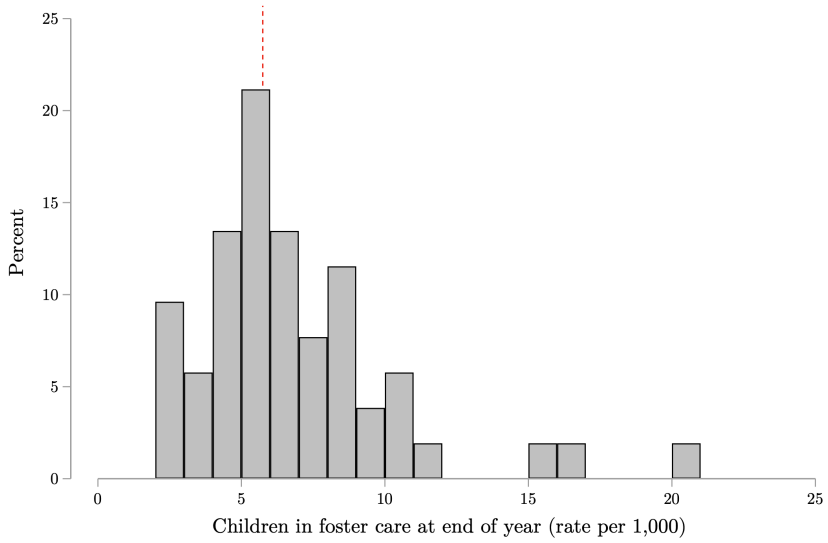
Similarly, while kinship care typically receives priority when placements are considered, states vary widely in their reliance on kin. Figure 3.1, Panel (b) shows kin placement rates range from less than 10 percent to nearly 50 percent across states. This variation in kinship placement stems in large part from differences in policy emphasis of recruiting and certifying kin, which mirrors the ongoing tradeoff between child protection and family preservation. Given the variation in child removal rates and placement types across child welfare systems, more evidence on the types of interventions that promote child well-being is needed.

For more detailed information on these trends and heterogeneity across states, Appendix C provides a series of figures. These include trends in child maltreatment, foster care placement, reasons for placement, and placement types over time and across states.

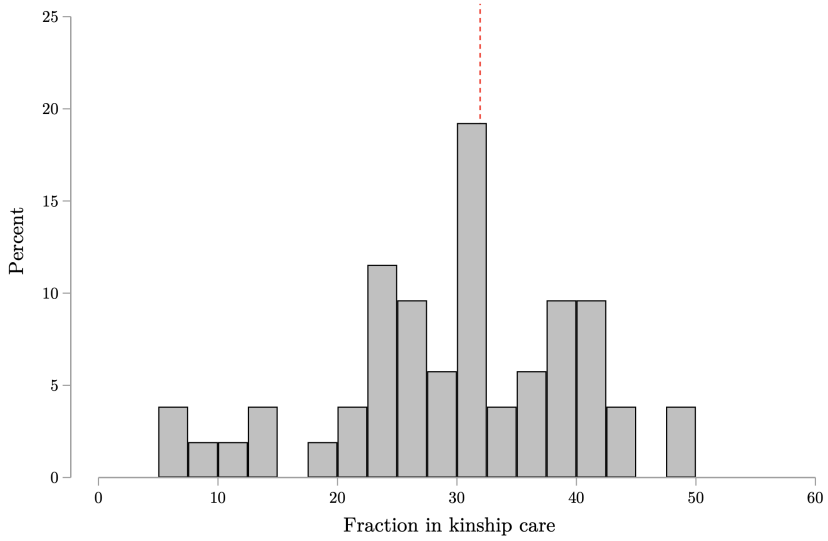
[6] Reported maltreatment rates explain about 15 to 28 percent of the variation in the percentage of children entering foster care across states. In a state-level regression of the percentage of children entering foster care in 2019 on the percentage of children subject to an investigated maltreatment report in 2019, the R-squared is 0.28. Using the percentage of children confirmed as victims of maltreatment gives an R-squared of 0.15.

Figure 3.1: *State variation in foster care placement rate and percentage kinship care*

(a) *Histogram of children in foster care at end of year (rate per 1,000)*



(b) *Histogram of percentage of children in kinship care*



Source: See text.
Notes: Panel (a) reports the foster care placement rate per 1,000 children (one observation for each state). We report placement rates among children ages 0 to 17 who were in foster care at the end of the 2019 fiscal year. The vertical line represents the nationwide placement rate. Panel (b) reports the percentage of foster children in kinship care (one observation for each state). We report the percentage of children ages 0 to 17 who were in foster care at the end of the 2019 fiscal year and who were placed in kinship care. The vertical line represents the nationwide percentage in kinship care.

3.3 Family Preservation versus Child Protection

When deciding on the stringency of child welfare policy, a central question is whether foster care placement improves child welfare *on the margin*. In other words, what is the impact of foster care placement for a child who was removed under one regime, but would *not* have been removed under an alternative regime? Although a well-documented correlation exists between foster care and poor outcomes for children even after controlling for observable differences across cases (for example, Berger et al. (2015)), it is challenging to identify causal effects because foster children differ in unobserved ways from children who were not placed. For example, foster children may have been more severely abused or neglected.

In the economics literature, the main empirical approach to overcome these confounding factors has been to consider the quasi-random assignment of child protective investigators to cases. When a report of maltreatment is made to a local child welfare office, a case worker is sent to investigate the report and determine if there is sufficient evidence of abuse or neglect. Many localities use a rotation process to assign cases to investigators working in the same field office to smooth the workload and often out of concern for fairness. In addition, despite assignment to similar cases, some investigators are more likely to recommend foster care placement compared to their colleagues. It is therefore possible that two children with very similar backgrounds will face different likelihoods of placement by virtue of receiving different (as-if randomly assigned) investigators. Such comparisons offered by this approach recover the causal effects of foster care for *marginal* children: those for whom the investigator assignment matters for the foster care placement decision. As a result, an important limitation of this approach is that it cannot determine effects for cases when all investigators would remove the child, nor for cases when no investigator would remove the child. Even so, understanding outcomes for marginal children provides insight into the effects of more- or lessstringent child welfare policies.

Doyle (2007b) used this approach to study children placed into foster care in Illinois during the 1990s. The study linked maltreatment and foster care data to state administrative data to measure a wide range of longer-term outcomes. The sample included children who were receiving welfare benefits (which facilitated matching across the outcomes) and who were aged 5–15 at the time of the initial investigation. The results imply large negative effects of foster

care placement on the margin, including substantially lower earnings and large increases in teen motherhood, delinquency, and unemployment. In later work, Doyle (2008, 2013) shows that foster care placement also increased the likelihood of both emergency medical care within a year of placement and criminal justice system involvement in adulthood. In similar work using the investigator strategy in South Carolina, Roberts (2019) finds that foster care placement roughly doubles the likelihood of juvenile delinquency over the five years following an investigation (an 11 percentage-point increase).

In contrast, positive effects have been found in other contexts. Gross and Baron (2022) studied school-aged children in Michigan subject to investigations from 2008 to 2016. The study finds that removal causes a significant reduction in the likelihood of subsequent maltreatment, as well as improvements in math test scores and school attendance. In related work, Baron and Gross (2022) find that removal significantly reduces adult criminality. In South Carolina, Roberts (2019) finds that foster care placement reduces the likelihood of repeating a grade by 13 percent within three years.

In addition, there is evidence that the effects of foster care placement vary across child subgroups, with younger children tending to benefit more. Bald, Chyn, et al. (2022) used linked administrative data from Rhode Island and found a positive impact of removal on measures of school achievement concentrated among girls removed before age six (they do not find such impacts for boys). Gross and Baron (2022) also find improved educational outcomes for younger children, while others find worse outcomes for older children in multiple settings (Doyle, 2007b, 2008; Warburton et al., 2014).

A possible reason for the contrasting findings across states and time periods is their different institutional contexts, as effects of foster care may vary with the threshold for placement and the quality of the foster care system. For example, foster care placements in Illinois in the 1990s were substantially longer and less stable than recent placements in Michigan.

Within a given context, studies have estimated the effects of foster care at different margins determined by different types of investigators. For example, among a subset of investigators with relatively low removal rates (more "lenient" investigators), the marginal child likely has relatively more severe maltreatment characteristics, as these investigators have a higher threshold before they recommend placement. Doyle (2007b) finds that the negative effects of foster care in terms

of teen motherhood are larger among children assigned to investigators with low removal rates and, consequently, marginal cases that have relatively severe maltreatment compared to cases investigated by higher-removal-rate investigators. Meanwhile, negative effects on labor market outcomes are found to be similar across investigator types, suggesting that the placement decision resulted in worse employment outcomes across the severity spectrum. In contrast to Doyle (2007b), Bald, Chyn, et al. (2022) find evidence of effects on achievement that are especially positive for young girls with higher unobserved maltreatment. This suggests that young girls with high risk levels experienced the highest gains.

One potential mechanism for the way involvement in child protective services affects children is how such involvement affects their family members, some of whom may be identified as "perpetrators" of maltreatment. The child protective services authorities provide a variety of services to families with the goal of preventing the need for removal, or in cases where removal is unavoidable, to support family reunification and decrease risk levels (Merritt, 2020). Gross and Baron (2022) find that in Michigan, children's gains from foster care placement occurred after most were reunified with their birth parents, which is consistent with the rehabilitation of birth parents while their children were in foster care. Similarly, using data from Allegheny County, Pennsylvania, Grimon (2021) finds that mothers were substantially more likely to enter drug treatment and receive mental health services not only at the time of the investigation, but for many years afterward.

Given the mixed evidence on the effects of placement for different margins and contexts, understanding the types of cases and child welfare interventions that promote child wellbeing is an important area for future research. One way to characterize the sets of issues involved is to focus on the demand and supply of foster care services.

3.4 Foster Care Demand

Demand for foster care comes from both the extent of child maltreatment in an area and policies that dictate how child protective services responds to it. As shown in Table 3.1, victimization rates have been relatively steady over the past 20 years, but the nature of maltreatment has moved away

from child abuse toward child neglect. Partly as a result, the ways in which states respond has changed over time as well, tending toward policies that aim to prevent the need for foster care.

3.4.1 Risk factors

Poverty is strongly associated with child maltreatment (for a survey of the literature, see Bullinger, Lindo, and Schaller (2021)). For example, numerous studies have shown that foreclosure, eviction, and other forms of housing insecurity, as well as food insecurity, increase reported child maltreatment. Less directly, financial resources can also influence parental stress, which could in turn lead to abuse or neglect.

Given these associations, efforts to reduce poverty, or to ameliorate its effects, can reduce demand for foster care. Experimental studies of welfare reforms suggest a causal relationship between family income and child maltreatment (Fein and Lee, 2003; Cancian, Yang, and Slack, 2013). These studies sometimes capture multiple policies (for example, changes in benefit levels as well as work incentives) and might not extend to different contexts. Quasi-experimental analyses of earned income tax credits across states find higher benefit levels are associated with a reduction in maltreatment rates, particularly for child neglect (Berger et al., 2017). Other studies find lower rates of maltreatment associated with investments in the social safety net.

Another leading driver of neglect is parental substance abuse, which has increased substantially over time as a fraction of maltreatment reports (see Table 3.1 earlier). Studies show that the ongoing opioid epidemic in particular has contributed to child maltreatment and foster care. Although it is challenging to measure the effects of the opioid epidemic, studies have leveraged changes in the features of the crisis, such as shocks to illicit drug prices, treatment, or opioid supply, and conclude that the opioid epidemic has increased foster care caseloads (for example, Hou (2021)). Taken together, the evidence indicates that policies outside of child welfare can influence the extent of child maltreatment and the demand for foster care.

The ways in which authorities from child protective services evaluate risk factors can influence demand as well. Historically, child protective services has used relatively rudimentary risk-modeling software to help caseworkers assess the potential harm a child faces by remaining at home. More recently, data analytics are showing promise in improving risk stratification, although

concerns about accuracy, equity, and fairness are prominent. In Allegheny County, Pennsylvania, the use of more sophisticated software to predict the likelihood of future out-of-home placement has been incorporated into the workflow of screening-in alleged maltreatment for further review. Efforts in Douglas County, Colorado, to display this kind of risk information to investigators are being studied with randomized controlled trials (Vaithianathan, 2019). As with other diagnostic tools, equity concerns are important to consider. In Allegheny, the introduction of decision support was associated with a reduction in the Black-White gap in screen-in rates—primarily as a result of increasing the percentage of White children that were referred for investigation (Goldhaber-Fiebert and Prince, 2019). Research will be needed to determine not only the efficacy of these tools, but also the potential existence of biases built into them (Drake et al., 2020).[7]

3.4.2 Family preservation

Child welfare agencies employ various strategies to reduce the demand for foster care by stabilizing at-risk families. The 2019 Family First legislation allows states to accept federal funds that were previously earmarked for foster care services and use them for prevention services. Over time, the goal is that such funding will become restricted to prevention services that have empirical support in one of three areas: in-home parent skill-based programs, mental health, and substance abuse. The Title IV-E Prevention Services Clearinghouse contains a list of approved programs (available at https://preventionservices.abtsites.com/). States also apply for federal waivers to experiment with new approaches (James Bell Associates, 2019).

Family First calls for each state to submit a five-year plan with proposed interventions for funding eligibility, which provides a glimpse of the types of programs that are popular to prevent foster care placement. For example, a common in-home parenting skills intervention is Parents as Teachers, an education program that aims to increase school readiness and reduce child maltreatment. The Nurse Family Partnership, a home visiting program that educates and supports new mothers, is common in part because experimental evidence shows the program results in a reduction of child neglect (Olds et al., 1997). For the other two categories of interventions,

[7] There are ongoing efforts to blind child welfare investigations to race by removing racial indicators and correlates from investigation reports (Baron, Goldstein, and Ryan, 2023). But some caution is warranted in taking this step, because blinding algorithms to indicators of race may increase inequity (Rambachan et al., 2020).

mental health and substance use, there are several trauma-informed, therapy-based interventions available. A common program is Functional Family Therapy, which includes weekly therapy sessions to build skills in youth and in the family as a whole.

A possible concern is that federal funding for family preservation programs could increase the surveillance of families. Monitoring families whose children remain in the home can induce fear that their children will be removed (Fong, 2020). Increased surveillance may also create challenges for studies that use maltreatment reports as an outcome by artificially increasing reports. As a result, evaluations of prevention programs should focus on a wide range measures of wellbeing, including health, education, and criminal justice involvement outcomes.

A more controversial approach to preventing foster care is known as diversion, in which child protective services encourages the informal placement of children with substitute caregivers, often kin, rather than entering the formal foster care system (Berrick and Hernandez, 2016). Opponents argue that diversion limits a family's access to services because non-licensed relatives do not qualify for the same monthly payments or family rehabilitation services as licensed foster parents. Advocates of diversion claim that the practice helps to keep families intact by allowing relatives, who may not meet restrictive licensing requirements, to care for children. A 2007 survey found that 39 states engaged in diversion (29 states actively promoted the practice), while 12 states prohibited it (Annie E. Casey Foundation, 2013). Recently, Family First provided funding for Kinship Navigator programs, which vary in implementation but aim to provide a middle ground between diversion and formal placement.

Understanding the effect of diversion can help inform the tradeoff between family preservation and child protection. Unfortunately, scant evidence exists on the effects of diversion. Chen et al. (2020) evaluated Safe Families for Children, a program that placed children with foster families supervised by a nonprofit agency, but without formal placement in the foster care system. Using a randomized encouragement design across cases, they found this program increased the share of children who had been returned home at one year, while not leading to greater subsequent reports of abuse.

3.4.3 Extending care to older ages

Demand for foster care can increase by extending the age of eligibility. Some 20,000 foster youth aged out of foster care in 2019 (based on the AFCARS data), and such youth may have few lifelines as they enter their 20s. Courtney et al. (2020) find that, among "emancipated" former foster youth in California, one in five previously dropped out of high school, one in four experienced homelessness, and nearly a third were incarcerated, all by the age of 23. One estimate suggests that the United States would save over $4 billion in lifetime costs if these youth had outcomes comparable to the general population (Annie E. Casey Foundation, 2019). The Fostering Connections Act of 2008 aimed to provide additional support for these youth by permitting states to use federal funds for foster youth beyond age 18.

A small body of descriptive and quasi-experimental research suggests that the push for extended eligibility provided a wide range of benefits to affected youth. Using nationwide data, Prettyman (2020) leverages the staggered roll-out of extended foster care across states. Using variation within states over time, the study finds that extended foster care reduces the likelihood of homelessness and incarceration, and increases high school graduation and employment. The California Youth Transitions to Adulthood Study (CalYOUTH) shows similar improvements in outcomes (Courtney, Okpych, and Park, 2018). Increasing demand for foster care services by expanding the eligible population to include vulnerable youth over the age of 18 appears to have been a wide-ranging policy success.

3.5 Foster Care Supply

The impacts of foster care also depend on the functioning of the child welfare system on the supply side. Considerations include the public or private supervision of foster care, ways of recruiting, licensing, and assigning foster homes, and reliance on different types of foster care.

3.5.1 Public and private supervision

Once children are placed in foster care, they are supervised by a separate set of caseworkers who specialize in recruiting and monitoring foster homes, reunifying families, and finding adoptive

homes. These tasks are often provided by private organizations (such as religious organizations). In the last few decades, several states have privatized some or all of their child welfare services. States award contracts to private non-profits and for-profit firms to assume responsibilities such as monitoring the safety of children in care.

Privatization can come with potential challenges, including stakeholder buy-in and accurate cost estimates (US Department of Health and Human Services, 2007). Hubel et al. (2013) studied Nebraska's effort to privatize its child welfare system, which involved replacing the traditional feefor-service model with a capitation amount based on an expected cost of service delivery.[8] The authors find that initial payment rates were too low, leading to substantial problems—namely, reductions in quality and availability of services along with a 27 percent increase in costs.

Performance-based contracts in state foster care systems show some promise, however. Such initiatives often include incentive payments (for instance, linking some portion of provider payment or future placements to measurable child outcomes). In addition, agency officials and providers review performance data, discuss client outcomes, and attempt to identify and institutionalize effective strategies for serving clients. Reilly et al. (2021) find that 25 states currently use performance-based contracting for at least one child welfare service. Several decades ago in New York, a randomized evaluation of a family reunification program, administered by private agencies on a capitation basis, found mixed results (Westat, Chapin Hall Center for Children at the University of Chicago, and James Bell Associates, 1998).

Although we are not aware of any recent experimental evaluations of performance-based contracting in the field of foster care, descriptive studies suggest that such initiatives may be effective. Garstka et al. (2012) report on demonstration projects that implemented performance-based contracting in Florida, Missouri, and Illinois between 2007 and 2010. They identify several metrics that improved after implementation, including increased child safety and successful discharge from care. Similarly, case studies of Tennessee and Rhode Island describe their experiences with performance-based contracting as successful (Lawler and Foster, 2013; Government Performance Lab, 2019).

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[8] Fee-for-service models in child welfare are structured so that the provider receives payments based on the specific services provided to foster children. A capitation model, in contrast, uses cost estimates to provide a fixed payment to the provider per child served, shifting more of the financial risk onto providers.

Regardless of the payment model, a better understanding of heterogeneity in value-added created by foster care agencies would inform improvements to the system. Quality dimensions at the state level are considered by federal Children and Family Services Reviews, which provide a blueprint for outcomes that might be considered. These include measures of safety (preventing foster care reentry), permanency (returning children home or providing placement stability), and child and family wellbeing. The wellbeing measures consider the receipt of needed services, although a wider set of outcomes could be included, such as educational outcomes and criminal justice involvement.

3.5.2 Foster home recruitment

Perhaps the simplest way to affect the supply of foster homes is to recruit more foster homes. Having a large number of willing foster homes can also result in better matching of foster children to homes (Hansen and Hansen, 2006). Modeling match quality using administrative data from Los Angeles, Robinson-Cortes (2019) shows that enlarging the pool of foster homes available to children would likely improve child outcomes. Recruiting more foster homes may also reduce the need for congregate care, which is seen as a last resort for children who cannot or should not be placed in a family setting (Chadwick Center and Chapin Hall, 2016).

The federal Children's Bureau recently assessed states on their foster home recruitment practices and rated 34 states as needing improvement (US Department of Health and Human Services, 2020b). While no national dataset exists to measure foster home capacity, the amount of foster homes available in some states outnumbers foster placements (Wulczyn et al., 2018). This suggests that there is not a shortage of foster homes everywhere, although child welfare agencies note that it can be difficult to find homes for subgroups like older children and children with special needs (Annie E. Casey Foundation, 2018). Agencies often rely on recruitment events at places of worship and advertising in partnership with other private organizations. Recently, digital and data-driven tools have been adopted to support recruitment efforts.[9] A better understanding of the methods available to modernize foster home recruitment may yield substantial gains in match quality.

[9] For example, see a description of New York's recruitment efforts at https://ocfs.ny.gov/programs/fostercare/recruitment/.

3.5.3 Foster home licensure

Table 3.2: *State foster home licensing requirements*

	States
Criminal history requirements:	
Background check and child abuse registry	50
Child abuse registry check for previous state lived in	35
Never convicted of assault or battery	13
Never convicted of a drug-related crime	9
Juvenile court records check	7
Individual requirements:	
Aged 21 or older	36
U.S. citizen or legal resident	14
Legally married if a couple	5
Ability to communicate in English	4
Training requirements:	
Initial licensing training	45
Annual training for license renewal	42
Training in First Aid/CPR	18
Home requirements:	
Separate bedrooms for opposite-sex children	37
Secure firearm storage	35
Non-smoking environment	26
Carbon monoxide detectors	20
Minimum square footage per child	18

Notes: State requirements come from the Child Welfare Information Gateway (2018, 2019a). Criminal history requirements are current as of September 2018. Individual, training, and home requirements are current as of February 2018. Note that some state child welfare systems are managed at the county level and may have stricter or more lenient requirements.

A key issue that affects the supply of foster homes is the set of requirements to obtain a foster home license. All states require prospective foster families to undergo criminal background checks, but important details of licensing vary across states. For instance, Table 3.2 shows that as of 2018, nine states disqualify applicants for any drug-related crime, 14 require citizenship, 18 have minimum square footage requirements, 18 require training in first aid, and 26 require a non-smoking environment. Further, foster parents undergo 23 hours of initial training on average, which varies substantially across states (the standard deviation of initial required training is 8

hours). They also complete 12 hours of annual retraining (with a standard deviation of 4 hours).[10] For kinship placements, relatives can be unlicensed in some states as long as basic safety standards, such as passing background checks, are met.

States do not receive federal subsidies for unlicensed homes, so they have incentives to encourage licensure. They may also have an incentive to lower licensure requirements, so the US Administration for Children and Families recently released a set of minimum quality standards for licensing foster homes, which includes six hours of pre-licensure parenting training, a written evacuation plan in case of an emergency, and assurance that the foster parents will not smoke in the presence of the child. Although the recommended standards are not binding, states are required to submit plans with a discussion of how and why their licensing standards deviate from the national standards (Children's Defense Fund, 2020).

It is not clear from economic theory how licensing requirements affect the wellbeing of foster children. On one hand, they establish minimum quality standards for specific aspects of care. On the other hand, licensing requirements may limit the supply of foster homes, which could increase placement in congregate care settings and reduce the quality of matches among those placed with families. In part because of the many requirements, it can take months for homes to obtain a license. Digital aids to complete and submit paperwork should increase the efficiency of the process. Regardless, further research on the effects of different licensing requirements is needed.

3.5.4 Monthly maintenance payments

Financial incentives can also affect the number of families willing to take in foster children. Foster families receive monthly subsidies to support the children staying with them, as well as one-time payments for specific needs. The average monthly foster care payment was just over $500, which is twice the amount of the average Temporary Assistance for Needy Families benefit (US Government Accountability Office, 2011). Payments vary widely across states, as well as by the age of the child, relative status, and other factors.

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[10] Authors' calculations (see Table 3.2 notes). Statistics on references, initial training hours, and annual training hours could be calculated for 44 states, 42 states, and 42 states, respectively. The remainder of statistics in Table 3.2 are derived from Child Welfare Information Gateway (2018, 2019a).

In theory, subsidies could act as a price mechanism to clear the market, but in practice, they reflect rules of thumb tied to the average cost of hosting one child. For instance, foster care subsidies often do not vary by location within a state or by placement setting, are not adjusted for inflation, and are revised infrequently (Goldhaber-Fiebert et al., 2014).

Some evidence suggests that these subsidies could be used as a policy lever. Doyle and Peters (2007) studied foster care supply and demand in the early 1990s. The study estimates that the monthly subsidy has a supply elasticity of 0.3 in states with low initial subsidy levels. In a related paper, Doyle (2007a) finds that when Illinois reduced the monthly subsidy to relatives by 30 percent, relatives were 15 percent less likely to offer care. The finding that foster homes respond to financial incentives is echoed in work on adoption subsidies, which shows that raising adoption subsidies increases adoptions relative to long-term foster care (for example, see Brehm (2021) and the references therein).

3.5.5 Effects of different types of placements

The type of placements that are employed is another supply-side policy that can affect child wellbeing. Foster care placements can vary in a number of ways: for example, kin or non-kin families, or residential versus family care. Non-kin families typically receive more training and vetting, and they are volunteering their services, which may lead to higher quality care. Meanwhile, kinship foster parents are typically offered fewer supports from child welfare agencies (Sakai, Lin, and Flores, 2011), though kinship care may benefit children by retaining family connections. Similarly, residential care may provide specialized mental or physical health services.

Evidence on the effects of foster placement type is limited, but some quasiexperimental studies shed light on the relative benefits of placement types. Doyle (2007a) examination of the reduction of monthly payments made to kin found that measures of quality of care, including wellness visits and placement stability, were not affected. This suggests that marginal kinship providers are similar to non-kin providers in terms of these quality measures. Hayduk (2017) studied state policy changes and found that children exposed to laws favoring kinship care experience more stable placements and shorter foster care episodes, but mental and physical health outcomes appear unaffected in the short run.

3.5.6 Independent living programs

When children transition out of foster homes or congregate care as young adults, independent living programs aim to help prepare them for adulthood. All youth ages 14 to 23 currently or formerly in foster care are eligible for independent living programs (Children's Defense Fund, 2020). Such programs include assistance in completing a high school diploma or GED, job training, or postsecondary education. Young adults exiting the foster care system who are at risk of homelessness are also eligible for housing vouchers provided by the US Department of Housing and Urban Development.

The federal government funded four experimental evaluations as part of the creation of the Chafee Foster Care Independence program enacted in 1999. Just one out of the four studies found positive impacts on key indicators of a successful transition to adulthood. The lone success story was from the Massachusetts Adolescent Outreach Program for Youths in Intensive Foster Care, which paired youths with a caseworker who met with young people once a week for 16 months to offer individualized services in areas such as job search. The program improved college enrollment and persistence but did not have impacts on other outcomes that it sought to influence, like employment and housing (Courtney et al., 2011). More recent evaluations of independent living programs show modest promise (for example, Skemer and Valentine (2016)).

3.6 Conclusion

Foster care is a far-reaching intervention in the lives of particularly vulnerable children. With substantial variation in how child welfare systems use the practice, and emerging evidence of heterogenous effects across systems, more evidence is needed to inform policy challenges and improve child and family wellbeing.

On the demand side, a better understanding of the drivers of child maltreatment, and interventions that ameliorate them, could prevent the need for foster care in the first place. This suggests that poverty alleviation programs, as well as programs aimed to improve the human capital of parents, should consider child maltreatment and foster care placement as outcomes in their evaluations. Further, targeted family preservation initiatives now receive federal subsidies, and there is an appetite for more evidence on the effectiveness of such programs. There is also a

need for better evidence on diversion programs that circumvent the formal foster care system, which are not only controversial but also understudied.

On the supply side, efforts to improve the quality of foster care through modernizing the recruitment and matching of foster homes are gaining some popularity and are in need of rigorous evaluation. Better measures of the supply of foster homes and signs of shortages, such as placement distance-from-home and time in emergency shelters, would help to evaluate the extent of any shortage problem and to gauge the effectiveness of efforts to solve it. For example, returns to different forms of targeted advertising, including appeals to child wellbeing, monthly subsidies, or information about the licensure process should be estimated to inform future recruitment strategies.

Some key questions about the best type of placement—non-kin, kin, and congregate care—for different types of children remain unanswered. For example, children currently residing in congregate care could have their placements revisited through more-intense case management, to test whether time in congregate care can be reduced and whether subsequent outcomes improve. In addition, the services provided to children already in foster care deserve more attention. Counseling, mentorship initiatives, and jobs programs should all be studied. At the agency level, much more could be learned about the impacts of private provision of services and performance contracting.

As agencies work to advance equity in child welfare and foster care, studying the effects of programs and policies are crucial next steps. For example, several jurisdictions have considered removing racial indicators and correlates from investigation reports when determining whether to place a child in foster care, yet research suggests caution is warranted in taking this step (Baron, Goldstein, and Ryan, 2023).

More generally, understanding the mechanisms driving heterogeneous effects of foster care is an important next step in addressing the underlying tradeoff between child protection and family preservation. The current patchwork of state and local foster care systems provides an opportunity for well-identified studies to understand the benefits and costs of such systems. Meanwhile, linked administrative data and foster care program data, after two decades of federal subsidies to modernize state child welfare data systems, are providing new opportunities to study the long-term impact of interventions. Coupled with a willingness among policymakers to generate

and employ evidence on best practices, foster care research is poised to inform policy and improve the welfare of children.

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Appendix A

Appendix to Chapter 1

A.1 Historical Appendix

A.1.1 Nurses in the complete-count census

Table A.1 reports statistics for nurses in the complete-count census from 1870 to 1930. During this period, nursing grew to become the largest occupation within the medical sector. The shift from private home nursing to hospital nursing is pronounced, especially between 1900 and 1910. By 1930, two thirds of nurses were categorized as professional, and 40 percent were working in a hospital setting. Though nurses were 91 percent female in 1870, this percentage increased to 98 percent by 1930. Nurses also became increasingly white and native-born. The age distribution of nurses shifted notably during this period. Before 1900, just 20 percent of nurses were aged 18 to 24. This share rose to 33 percent by 1930. Much of this shift is due to the spread of nurse training, which lowered minimum age requirements gradually during this period. As high school graduation rates rose, more women entered nurse training directly after high school (Goldin, 1998). The share of all nurses who were currently married or had children did not change sharply during this period, though the increase in marriage rates between 1920 and 1930 is notable.

I use a linked sample of nurses to describe who became a nurse from 1870 to 1930. This analysis uses FamilySearch links published by Buckles, Haws, et al. (2023). I take a backward-linking approach, matching nurses in the complete-count census backward to childhood. This allows me to study the background characteristics of nurses in the population. Table A.2 reports match rates across census years for female nurses ages 15 to 64. Match rates are higher in later censuses. Native-born women are matched at higher rates than foreign-born women, in part because foreign-born women may not be present in the earlier census. Figure A.3 shows the percent of nurses (ages 18 to 25) from different family backgrounds, measured using father's occupation in childhood. Young nurses became increasing selected from families with professional fathers during this period. This share rose from less than 10 percent of young nurses in 1870 to 15 percent of young nurses in 1930. Women also became less likely to come from manual or farming backgrounds, though these women still represent a large share of nurses.

A.1.2 Alternatives to nursing

Patterns of occupational sorting are key to understanding the effects of access to nurse training. Many women likely had other careers in mind, yet there are no historical sources that ask women about the occupation they would have chosen had they not trained as nurses. In a report sponsored by the Indiana University School of Education, Blazier (1924) surveyed 250 trained nurses and asked about prior occupation. Only 35 percent of these women reported a prior occupation. Half of women with a prior occupation worked in teaching, and one third worked in the clerical sector. A more extensive survey was conducted by the Committee for the Study of Nursing Education, sponsored by the Rockefeller Foundation in 1918 (Committee for the Study of Nursing Education, 1923). Of the 15 training schools surveyed with records of prior work for their students, the most common prior industries were teaching, clerical, or healthcare (*e.g.*, children's nurse, dental assistant). I explore the effect of proximity to nurse training on entrance into different occupations in Section 1.3.3.

A.1.3 Regulation of nursing

Nursing became a regulated occupation in the first decade of the 20th century. In North Carolina, Mary Lewis Wyche, a nurse, is thought to have initiated the push for regulation by founding North Carolina's State Nurses Association in 1902. In 1903, North Carolina passed a certification law requiring that all nurses who wished to practice under the title of "registered nurse" (R.N.) pass an examination administered by the state nursing board. Successful applicants could practice as R.N. in any county in the state, and would be included in the county clerk's register of trained nurses. Use of the title of R.N. without proper paperwork was punishable by a $50 fine or no more than 30 days imprisonment (Pollitt and Miller, 2010).

Within one year, New Jersey, New York, and Virginia followed the example of North Carolina. Figure A.4 plots the cumulative adoption of laws concerning registered nurses in the U.S. from 1890 to 1940, sourced from Carollo (2024). All states passed laws regulating nurses by 1940. By contrast, midwifery was regulated by 20 percent of states in 1900 and 60 percent of states in 1940. Figure A.5 plots the year a certification law for registered nurses was adopted in each state. Early adopters were more likely to be mid-Atlantic states, and late adopters were more likely to be

states that recently joined the Union. In Table A.3, I identify the state characteristics that predict certification law adoption. Column (1) reports the results from a regression of adoption year on characteristics measured in 1900. Population and literacy emerge as statistically significant predictors, with larger and more literate states adopting earlier. In columns (2) and (3), I separately identify predictors of state adoption in the 1900s and the 1910s. The estimated coefficients largely align with column (1), though only literacy is statistically significant in these specifications. These findings broadly align with Carollo et al. (2022), who find that large labor markets and the presence of state associations predict the passage of licensing laws in many occupations.

Laws from this period were permissive: Individuals who didn't obtain certification could still practice as a nurse under a less formal title like "practical nurse" or "nursing aide" (Law and Kim, 2005; Law and Marks, 2017). It was common for the laws to include provisions for older nurses who were trained under different standards. For example, Massachusetts passed its certification law in 1910 and stipulated that nurses with at least five years of nursing experience could become registered without a formal examination. Nurses who moved across state lines could remain certified, so long as the standards of the previous state were acceptable in the new state. Though certification was voluntary, advocates for reform viewed these laws as furthering the professionalization of nurses (Shannon, 1975). Opposition to these laws was common among untrained nurses and graduates of low quality schools, who were fearful of losing work. Additionally, physicians were concerned that the recognition of nursing as a regulated profession would threaten their authority and embolden nurses to push for further reform (Rosenberg, 1987).

As the medical field developed, new nurse categories emerged and states passed stronger regulation. Licensed practical nurses (LPNs, also known as licensed attendants) emerged out of a growing need to supply basis nursing services during the interwar period (Deming, 1950). States began regulating practical nursing in the 1920s (Carollo, 2024). In the 1940s and 1950s, states shifted from permissive to mandatory nurse licensure, and literature suggests that this shift increased nurse wages and didn't affect minority participation (Law and Marks, 2017). The certified nursing assistant (CNA) formalized as an occupation during the mid to late 20th century, when community colleges and vocational schools began offering CNA certification alongside other nursing programs.

Figure A.1: *Student nurses in Indianapolis, IN*

Source: Blazier (1924).

Figure A.2: *Questions for admission to nurse training programs in 1898*

XIII.

QUESTIONS ASKED OF CANDIDATES FOR ADMISSION AS PROBATIONERS.

(No one school asks all of these questions, but each of them is asked by some one school, and most of them by all schools.)

PHYSICAL.

1. Name in full of candidate (not initials or any pet name).
2. Address, and nearest telegraph station.
3. Age. Date and place of birth.
4. Color.
5. Height.
6. Weight.
7. Bust measure.
8. Color of hair.
9. Color of eyes.
10. Are you, or have you been, lame?
11. Do you wear glasses? if so, why?
12. Are your sight and hearing perfect?
13. Are both your parents living? If not, what was the cause of death?
14. Have you, or have you had, any uterine disease?
15. Have you any physical defect or blemish?
16. Are your teeth in good condition?
17. Have you any known tendency to any disease, particularly of the lungs?
18. What illnesses have you had—particularly rheumatism, pneumonia, measles, scarlet fever, quinsy or other throat disease?
19. How lately have you been under a doctor's care, and for what disease?
20. Have you been successfully vaccinated, and when?
21. Do you use liquor, opium, or other drugs, or tobacco?
22. Are you strong and healthy, and have you always been so?

EDUCATIONAL.

23. Where were you educated?
24. At academy, high school, or college?
25. If college-bred, have you a diploma?

26. Have you taught school?
27. What books have you read?
28. Can you speak any foreign languages?
29. Can you speak English correctly?
30. Can you write legibly?
31. Have you knowledge of Arithmetic as far as percentage?
32. Do you sing, or play any musical instrument?
33. Do you sew well?
34. Have you any knowledge of housework?

PERSONAL.

35. Are you single, engaged, married, divorced, or widowed?
36. Have you children? How many? Ages? How are they provided for?
37. Have you any one dependent on you?
38. Have you any private means of your own?
39. What is your religious belief?
40. What is your denomination?
41. In what denomination were you baptized?
42. Are you now a regular communicant?
43. Give name and address of your clergyman.
44. Are you free from responsibility, so that you are not likely to be called away?
45. What is your present occupation or employment, if any, outside of household or domestic duties?
46. Why do you wish to leave it?
47. Former occupation for five years?
48. Your father's occupation?
49. Name and addresses of two (or three) persons, (one at least a lady), not relatives, who have known you at least two years.
50. Have you full consent of your parents or guardians to enter this school?
51. Are you now negotiating or under engagement with any other school?

PROFESSIONAL.

52. Have you had any special experience in nursing?
53. Have you ever nursed before, or been in any Training School?
54. Have you ever been rejected by any school? What one, and why?
55. Have you nursed in private families?

56. Have you had any experience in nursing the insane, and in what capacity?
57. Do you intend following nursing as a profession?
58. Do you object to the menial duties inseparable from the profession?
59. Have you fear of any disease to the care of which you may be assigned?
60. Are you willing to act as Night Nurse?
61. Are you willing to take charge of male patients (if necessary) to begin with?
62. Are you willing to wear the uniform of this school, after graduation, when on duty?
63. Do you promise to maintain, whenever on duty, the cleanliness and order of the ward in which you are serving, and to implicitly obey the orders of your superiors in charge?
64. If accepted, do you promise to remain throughout the entire course of two (or three) years?
65. Have you read, and do you clearly understand and agree to all the stipulations and conditions herein contained?

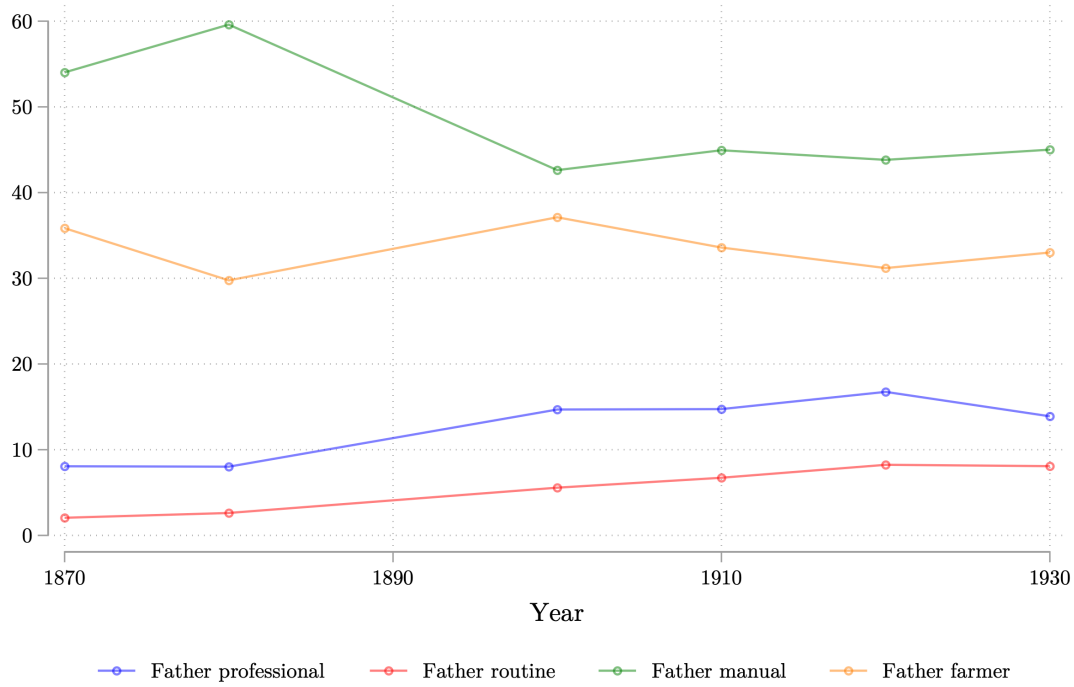
Candidates should also furnish a physician's certificate of health, which has sometimes to be supplemented by a physical examination by the school's medical examiner. They are also frequently examined in the ordinary branches of an English education.

A photograph of applicant is also frequently required sent with application.

Always two, sometimes more persons, not related to applicant, are required to endorse her application. It is frequently required that one of these be a clergyman.

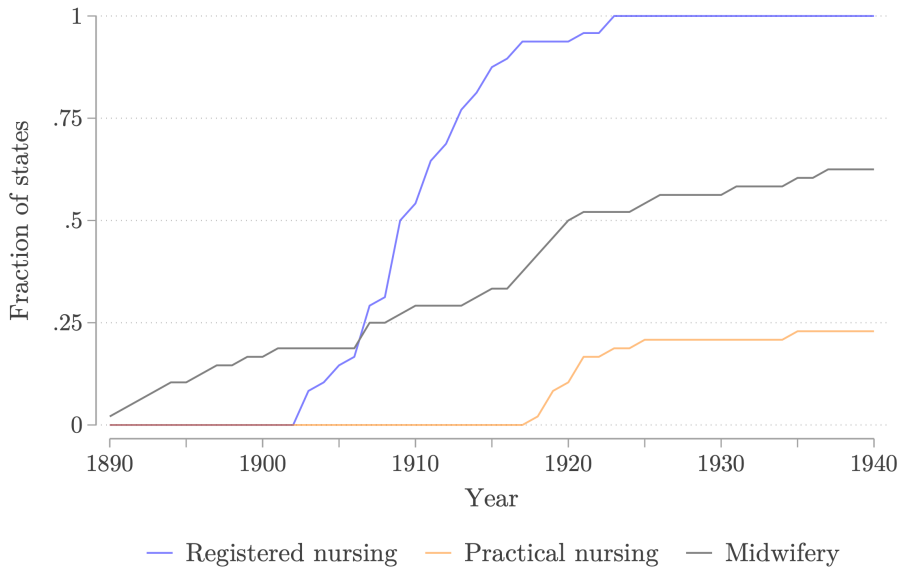
Source: Hodson (1898).

Figure A.3: *Background of nurses over time, 1870 to 1930*



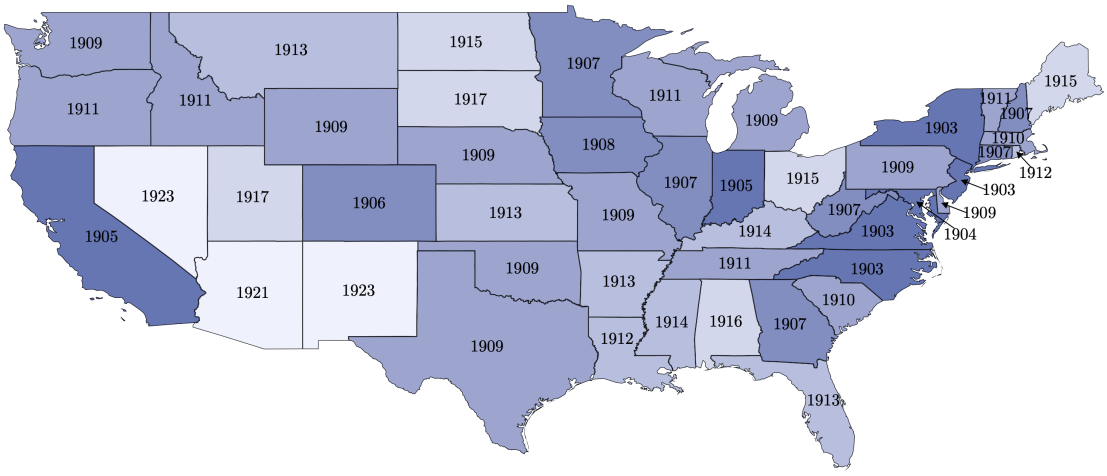
Notes: Figure reports the background of nurses in the census from 1870 to 1930. Nurses are linked backward to their childhood census using FamilySearch crosswalks (Buckles, Haws, et al., 2023). Background is proxied using father's occupation, as observed in the most recent childhood census at ages 0 to 15. Father's occupation is grouped into four categories using the occupational scheme of Song et al. (2020): professional, routine (non-manual), manual, and farming. The sample is limited to all nurses at ages 18 to 25.

Figure A.4: *Adoption of certification laws for registered nurses*



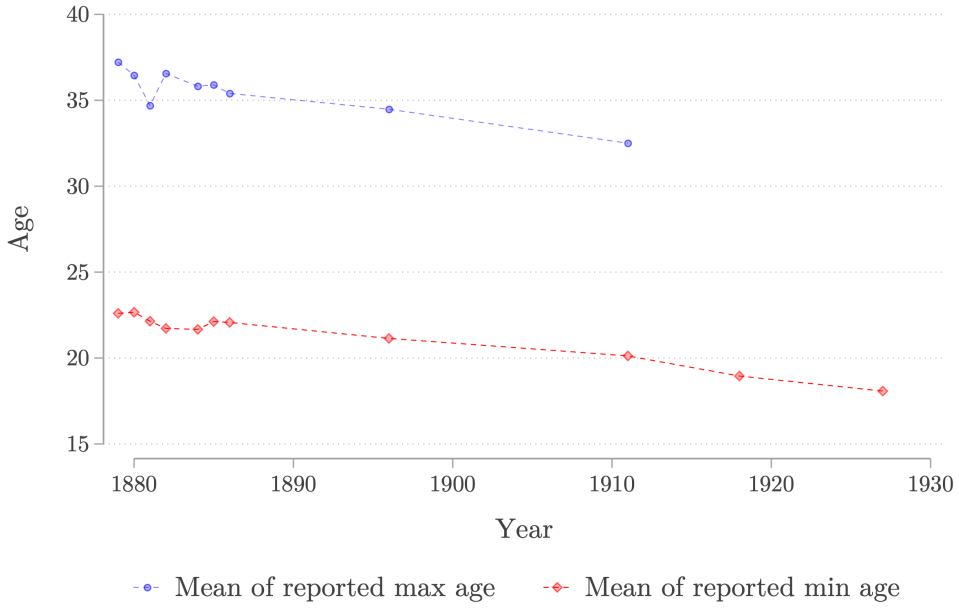
Notes: Figure reports the fraction of states with any law regulating the practice of registered nursing, licensed practical nursing, or midwifery from 1890 to 1940. Laws regulating registered and practical nurses are sourced from Carollo (2024). Laws regulating midwives are sourced from Anderson et al. (2020). Alaska, the District of Columbia, and Hawaii are excluded.

Figure A.5: *Timing of certification laws for registered nurses*



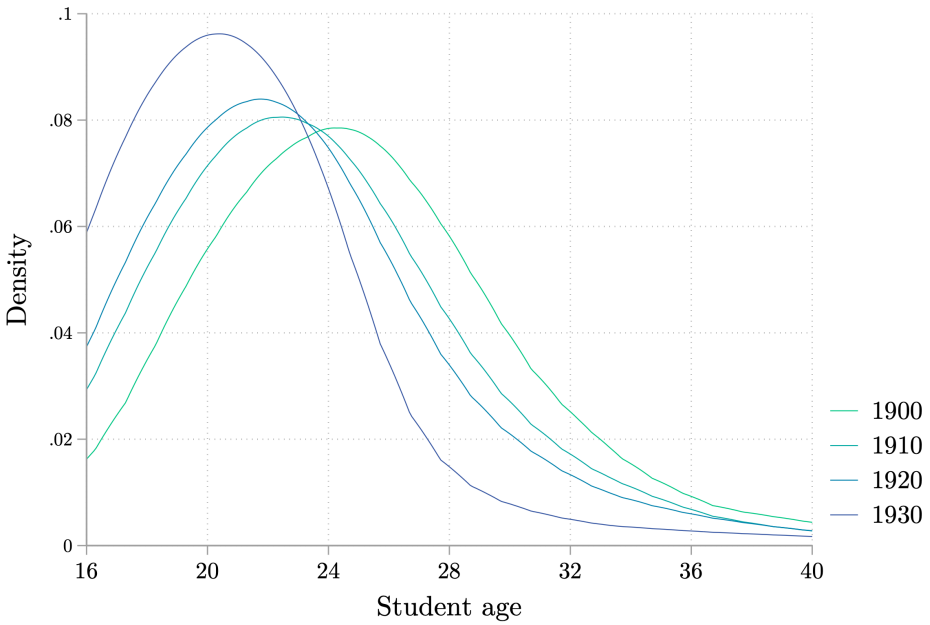
Notes: Figure plots state adoption of certification laws for registered nurses. Year of first adoption is reported, with lighter colors indicating later adoption years. Laws regulating registered nurses are sourced from Carollo (2024). Alaska, the District of Columbia, and Hawaii are excluded.

Figure A.6: *Reported minimum and maximum ages for student nurses*



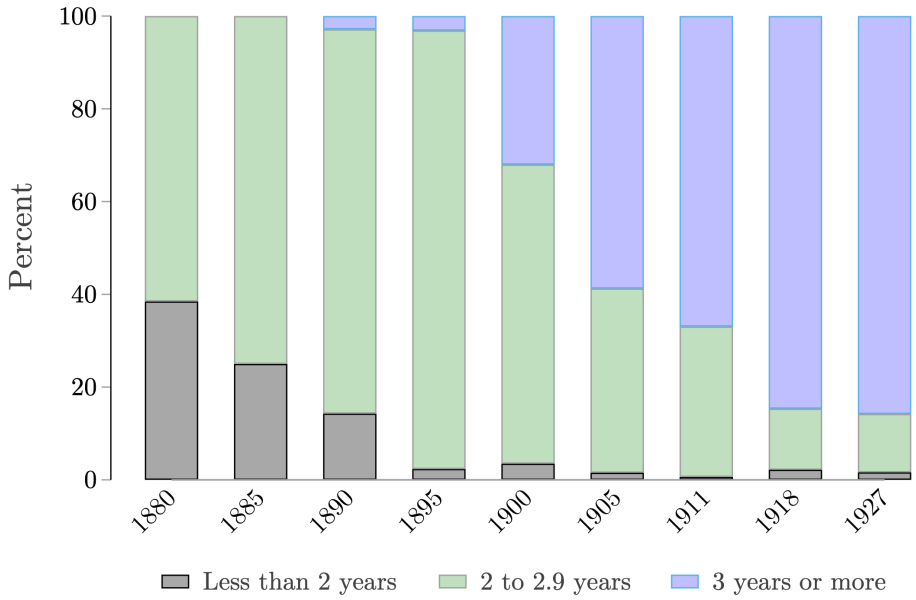
Sources: U.S. Bureau of Education. See Appendix Section A.3.1.
Notes: Figure shows the minimum and maximum ages that nurse training schools accepted over time. The mean in each year is shown. Maximum ages were not reported after 1911.

Figure A.7: *Age distributions of student nurses, 1900-1930*



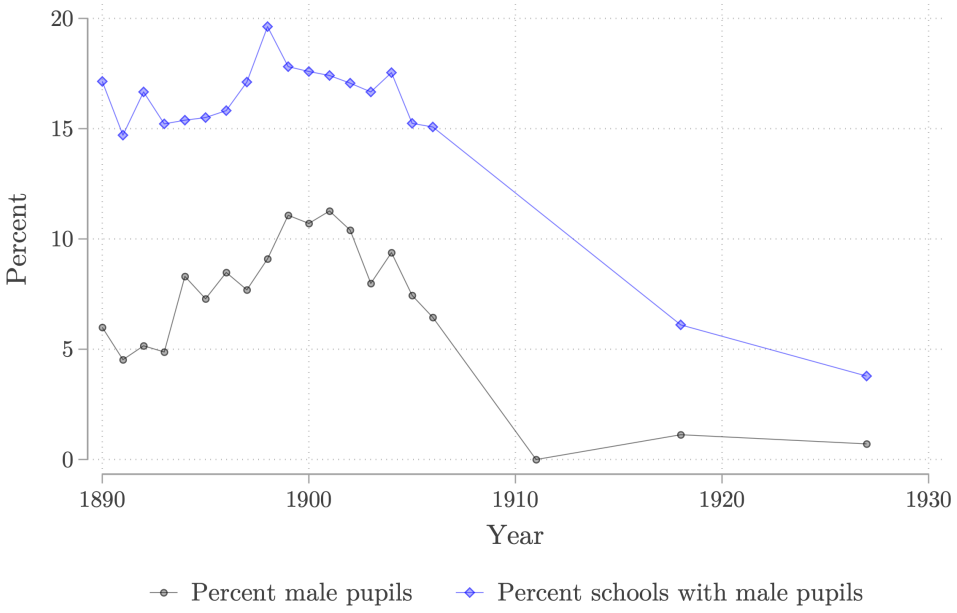
Notes: Figure shows the age distribution of student nurses in each complete-count census from 1900 to 1930. Sample is limited to white, female, native-born student nurses (1950 occupation code of 59). Ages under 16 and over 40 are binned at 16 and 40, respectively. The distribution is plotted separately for each census, estimated using kernel density with bandwidth of 3.

Figure A.8: *Length of nurse training over time*



Sources: U.S. Bureau of Education. See Appendix Section A.3.1.
Notes: Figure shows the reported length of nurse training over time. Length of training is grouped into three bins: Less than 2 years, 2 to 2.9 years, and 3 years or more. The percent of schools that fall in each bin is reported in each year.

Figure A.9: *Male nursing students over time*



Sources: U.S. Bureau of Education. See Appendix Section A.3.1.
Notes: Figure shows in black the percent of students who were male in each year, aggregated across all training schools. The percent of schools in each year that had any male students is shown in blue.

Table A.1: *Descriptive statistics of nurses in the census*

	(1)	(2)	(3)	(4)	(5)	(6)
	Census year					
	1870	1880	1900	1910	1920	1930
Nurses (count)	11,058	23,406	88,346	178,018	250,563	417,344
Percent of medical workers	11.8	16.4	31.9	44.3	50.2	59.2
Subcategories (percent)						
Trained nurses	1.4	1.7	12.3	43.8	51.8	65.7
Practical nurses	98.6	98.3	87.7	56.2	48.2	34.3
In hospital	1.2	1.2	2.4	23.4	29.3	38.9
In private home	95.6	98.3	86.3	48.3	38.3	34.3
Demographics (percent)						
Female	91.0	94.8	92.4	94.3	96.1	97.7
White/non-Hispanic	71.8	69.7	82.3	87.7	93.1	94.2
Foreign-born	28.6	25.4	23.4	22.0	16.8	13.8
Ages 15-17	16.5	14.0	7.0	4.3	1.9	1.3
Ages 18-24	18.7	20.1	22.8	24.6	26.3	33.5
Ages 25-34	15.4	14.6	27.2	29.8	29.5	24.4
Ages 35-44	15.4	14.6	16.8	18.0	18.8	17.5
Ages 45-65	34.2	36.7	26.1	23.2	23.4	23.3
Currently married	7.7	13.1	12.9	14.2	13.8	17.4
Has children	23.1	24.2	18.5	16.5	13.8	13.3

Notes: Table reports descriptive statistics (means) for nurses in the complete-count census from 1870 to 1930. Sample includes all nurses aged 15 to 65. Nurses (count) includes trained and practical nurses. Medical workers includes nurses as well as the medical professions (dentist, optometrist, osteopath, pharmacist, physician).

Table A.2: *Match rates of female nurses across census years*

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Age eligible									
					Linking year				
N	Base year	1850	1860	1870	1880	1900	1910	1920	1930
8,730	1870	0.216	0.204						
19,498	1880	0.265	0.254	0.299					
74,037	1900	0.242	0.277	0.324	0.361				
149,997	1910	0.223	0.252	0.291	0.343	0.392			
228,740	1920	.	0.277	0.296	0.339	0.410	0.449		
362,517	1930	.	.	0.294	0.330	0.374	0.422	0.444	
413,525	1940	.	.	.	0.341	0.366	0.399	0.447	0.513
Panel B: Age eligible U.S.-born									
					Linking year				
N	Base year	1850	1860	1870	1880	1900	1910	1920	1930
6,294	1870	0.253	0.213						
14,538	1880	0.305	0.276	0.321					
56,691	1900	0.313	0.335	0.390	0.432				
116,786	1910	0.287	0.311	0.354	0.421	0.440			
190,889	1920	.	0.334	0.360	0.411	0.460	0.484		
311,902	1930	.	.	0.358	0.403	0.436	0.469	0.479	
377,860	1940	.	.	.	0.414	0.421	0.440	0.472	0.525
Panel C: Age eligible foreign-born									
					Linking year				
N	Base year	1850	1860	1870	1880	1900	1910	1920	1930
2,436	1870	0.136	0.181						
4,960	1880	0.177	0.202	0.233					
17,346	1900	0.045	0.106	0.138	0.152				
33,211	1910	0.031	0.048	0.088	0.112	0.226			
37,851	1920	.	0.018	0.047	0.078	0.171	0.275		
50,615	1930	.	.	0.029	0.042	0.116	0.156	0.225	
35,665	1940	.	.	.	0.020	0.076	0.138	0.199	0.386

Notes: Table reports match rates of female nurses aged 15-64 across census years. Each base year is linked to prior census years using the FamilySearch crosswalks (Buckles, Haws, et al., 2023). In Panel A, the sample includes nurses old enough in the base year to be linked (for example, nurses aged 20 and up for the link from 1940 back to 1920). In Panel B, I restrict the sample in Panel A to native-born nurses. In Panel C, I restrict the sample in Panel A to foreign-born nurses. I report total nurses in the base year in the left-most column. Each cell uses a different subset of age-eligible observations.

Table A.3: *Predictors of nurse certification law adoption*

	(1)	(2)	(3)
	Year adopted	Adopted 1901-1910	Adopted 1911-1920
Ln(population)	−2.115***	0.143	0.246
	(0.667)	(0.089)	(0.163)
Percent black	−0.006	−0.018	0.023
	(0.137)	(0.016)	(0.020)
Percent urban	0.012	0.001	0.001
	(0.064)	(0.008)	(0.004)
Percent foreign-born	0.032	−0.011	0.010
	(0.105)	(0.015)	(0.022)
Percent literate	−0.276***	0.015	0.040**
	(0.075)	(0.014)	(0.012)
Percent women employed	−0.116	0.037	−0.024
	(0.265)	(0.035)	(0.027)
Ln(physicians per capita)	3.149	−0.233	−0.780*
	(2.663)	(0.307)	(0.367)
Ln(nurses per capita)	−1.034	0.086	−0.216
	(1.044)	(0.155)	(0.247)
Training schools per 100k	−0.998**	0.085	0.044*
	(0.383)	(0.063)	(0.021)
Women's suffrage law	−1.541	0.072	−0.178
	(2.181)	(0.295)	(0.227)
Region FE	Yes	Yes	Yes
Mean outcome	1911	0.54	0.86
F-statistic	16.048	6.926	46.532
Observations	48	48	22

Notes: This table reports the predictors of nurse certification law adoption at the state level. Each column shows results from a multivariate regression estimated using OLS, where the dependent variable is the year in which a state first adopted a nurse certification law (column (1)), an indicator for adoption from 1901 to 1910 (column (2)), and an indicator for adoption from 1911 to 1920 (column (3)). Explanatory variables are measured in 1900 for columns (1) and (2) and 1910 for column (3). Columns (1) and (2) include all states (excluding Alaska, Hawaii, and the District of Columbia), and column (3) restricts to states that had not adopted a law by 1910. Percent and per capita variables are measured using the entire state population. Percent women employed is measured using women aged 15-64, where employment is defined in Appendix Section A.3.2. Training schools is measured as the count of schools per 100,000 women aged 15-64. The mean of the outcome is reported, along with the *F*-statistic from a joint test of significance for the variables shown. All specifications include region fixed effects. Heteroskedasticity-robust standard errors in parentheses.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

A.2 Case Study of Nurse Training School Alumnae

Institutional archives are a rich source of data and contain information not found in other sources. Prior to 1940, the decennial census did not record information on educational attainment for adults. In 1940, individuals reported years of education, but didn't provide information on type of degree or institution. In addition, the decennial nature of the census makes it difficult to capture short-run labor market dynamics. Archival records therefore fill a crucial gap in knowledge about the employment patterns of trained nurses.

A.2.1 Archival records from three training schools

I access and digitize the archival records of nurse training schools. Training schools kept updated lists of graduates, similar to colleges and universities.[1] These records were published in annual reports that listed both current graduates and information on prior alumnae. An example of the original records is shown in Figure A.10. From these data, I observe a nurse's maiden name, married name, graduating cohort, location, and occupation. By comparing alumnae information in published reports that are updated over time, I can answer questions like: *What are the rates of marriage for graduates over time? What share of graduate nurses work in private homes or in hospitals?* This approach relies on graduates updating their information, which they frequently did.[2] One reason is that new graduates frequently took jobs as private or visiting nurses. The training school could refer work to these women using its register of graduates, and so it was important for nurses to keep their contact information up to date. A second reason is that each school had an alumnae association, which mailed announcements as its primary form of communication.

I collect and digitize records from three nurse training schools: the Bellevue Hospital Training School for Nurses in New York City, the Presbyterian Hospital School of Nursing in Chicago, and the Massachusetts General Hospital School of Nursing in Boston (henceforth Bellevue, Presbyterian, and MGH). These schools were chosen because their records were previously scanned and made public. These schools have the added benefit of being prominent, geographically diverse institutions: Bellevue and MGH were two of the earliest and most prestigious schools,

[1] See Bleemer and Quincy (2022) for an example of historical records from California universities.

[2] Only 5 percent of women report no information after graduation, and half update their information at least once.

both founded in 1873. Presbyterian was founded in 1903 as part of Rush Medical College. The reports contain graduates of Bellevue from 1875 to 1919, graduates of Presbyterian from 1906 to 1932, and graduates of MGH from 1875 to 1939.

To generate usable data from the scanned records, I use the functionality provided by LayoutParser (Shen et al., 2021). Optimal character recognition is used to extract the text from each page. I use Faster R-CNN to match text to fields in the portions of the records that have cleanly delineated columns. The remaining text is sorted into fields based on page location and regular expressions. I combine the three schools to form a dataset of nurse training school graduates over time, where the length of time I can see a given graduating cohort is limited by the available data. For instance, the Bellevue records end in 1919, so I only observe the graduating class of 1919 once. Table A.4 summarizes the processed records for each school and includes the dates of hospital and nurse training school founding. The full dataset contains 44,429 observations of 5,499 graduates. Without a broader data collection effort, it is difficult to know how these records compare to the average training school. Given that Bellevue and MGH were understood to be high-quality institutions, I expect the following results to be positively selected in terms of career outcomes.

From these data, I construct several outcomes. *Married* is an indicator for whether a woman was listed with a change of surname (for example, in Figure A.10, "S. W. Adams . . . Now Mrs. F. W. Conrad"). I allow this indicator to change over time, for instance if a woman is widowed and returns to work. I therefore also define *Ever married* as an indicator for whether a woman has ever been married by a given age (with *Never married* as the converse). *Employed* is an indicator for whether a woman is listed with an occupation. This measure includes military service and excludes women recorded as attending school or at home. I impute age and birth cohort using graduation year, assuming all women graduated at 22 years of age (Committee for the Study of Nursing Education, 1923). While this assumption is consistent with some historical records, I may be underestimating age, especially for early cohorts.

A.2.2 Patterns of marriage and employment

I characterize the marriage patterns of nurses relative to broad trends in the female labor force. In this exercise, I cannot explicitly subset by race or nativity. However, nurses were fairly homogeneous during this period: 90 percent of professional nurses were white women (Figure A.20), and roughly 25 percent were foreign-born (Table A.1). Note that “deceased" is an outcome listed in the records, so statistics are not conditional on survival. I first collapse the data by 10-year age bin and decade observed. For instance, a woman who graduated from Bellevue in 1880 would appear in 1890 with an imputed age range of 30-39. Table A.5, Panel A reports marriage rates for graduate nurses observed in each decade from 1890 to 1940.

In 1890, 13 percent of graduate nurses ages 20 to 29 were recorded as married. By 1940, this rate has increased to 39 percent, which reflects the decline in age at first marriage documented by Goldin (2021). It is informative to plot the fraction of graduates never married, to allow for a more direct comparison with prior literature. As shown in Figure A.11, the fraction never married falls across all age groups from the 1870 birth cohort to the 1890 birth cohort. Even though the main cohorts I observe are from a slightly earlier period, the downward trend is consistent with the cohorts studied by Goldin (2021) (page 34). Note also that there are several differences that make a direct level comparison difficult, including that these women are not college educated and may have different preferences for marriage.

In Table A.5, Panel B, I report employment among currently married women. In the archival records, these women were listed with a married name but also reported an occupation. Rates of employment while married are very low. I calculate that 50 percent of ever married women exited the labor force entirely upon marriage, and roughly 30 percent never worked and married soon after graduation. Prior to 1940, rates of employment among married women are typically higher at older ages. In 1940, the decline reverses as more married nurses remain in the workforce. These patterns line up with Goldin (1990), who reports a participation rate of roughly 20 percent among married women in the 1906-1915 birth cohorts (page 121). I additionally calculate that 50 percent of graduates exit the labor force entirely upon marriage

Table A.5, Panel C shows employment among women never recorded as married. I observe very high rates of participation in the labor force for the 20-29 age range (between 65 and 85

percent). This is not surprising given that this sample of women received two to three years of medical training that was persistently in high demand from hospitals and households. These numbers are almost uniformly higher than Goldin (1990), which reports participation rates of 53 and 68 percent for similar women in 1890 and 1920 (page 18). This evidence suggests that even among a group of medically trained and highly selected women, we see the harbingers of broader changes in the labor market. Graduates born between 1860 and 1870 have very low marriage rates, but beginning with the 1870 cohort, a rising number of graduates are married in their twenties. A portion of this change will reflect selection into nursing: As nursing schools spread and working hours were lowered, nursing became a more attractive option and less tied to its laborious, monastic origins (Rosenberg, 1987). By the 1900 birth cohort, the fraction of unmarried graduates at age 40 had fallen from 0.70 to 0.45.

A.2.3 Types of employment

Next, I investigate what fraction of nurses worked in different occupations. I construct indicators for employment in private homes and businesses, hospitals, and the military. I also identify women working in a managerial role (such as training school superintendent). These categories are not defined to be mutually exclusive. Conditional on reporting any occupation, Figure A.12 plots the fraction of nurses aged 20-39 in these categories over time. Before 1900, the majority of graduates entered work as a private duty nurse in either homes or businesses. With the rise of larger, more modern hospitals, the profession shifted from private to hospital employment. Between 10 and 20 percent of young nurses took on managerial roles, which reflects the need for leadership during a period when training schools were expanding. Bellevue and MGH were considered top-tier programs, so more of their graduates took on leadership roles within nursing relative to Presbyterian. The majority of records in the 1930s come from Presbyterian, which explains the drop in managers observed in these years. Finally, we see the impact of World War I on nursing employment in the military. In the MGH alumni records, nearly 40 percent of the MGH graduating classes of 1915-1917 spent time in the military, primarily as a nurse in the Army Nurse Corps (Kalisch and Kalisch, 2004).

A.2.4 Locations of graduates

Finally, I characterize the locations of graduates. Note that I do not observe a graduate's hometown, just reported location after graduating from nurse training school. Between 50 and 70 percent of graduates remained in the state where they trained, a percentage that remains relatively stable as the cohorts age. City of residence isn't always recorded, but the MGH records contain the best coverage of this variable. Over time, between 25 and 45 percent of MGH alumnae were located in the city of Boston proper, not including the surrounding towns. This finding is consistent with prior literature that shows how cities offered women better independent work and marriage prospects than small towns (Withrow, 2021).

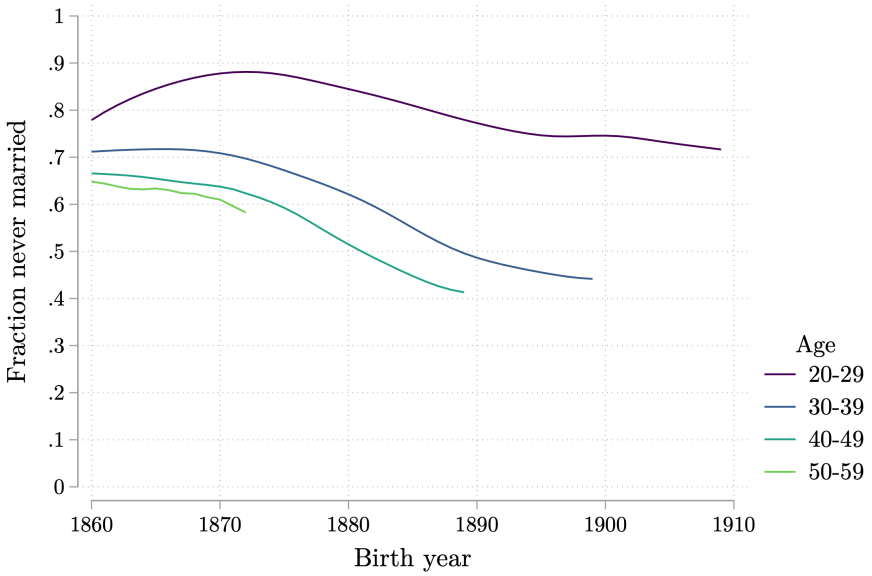
Figure A.10: *Sample of Bellevue Training School records*

LIST OF GRADUATES FROM 1875 TO 1886

Name.	Graduated.	Occupation.	Residence.
Miss A. E. ANDREWS	1878	Matron, City Hospital	Lawrence, Mass.
" J. J. ANGELL	1879	Private nurse	New York City.
" E. A. ALDRICH	1880	District nurse	" "
" S. E. ALLEN	1881	Matron, lying-in hospital	London, Eng.
" S. W. ADAMS	1882	Now Mrs. F. W. Conrad, at home	Santa Barbara, Cal.
" A. M. ALTON	1884	Private nurse	New York City.
" JENNIE ARTHUR	1884	At home	Brynmaur, Pa.
" H. M. ALFORD	1882	Now Mrs. Devine, at home	New York City.
" LUCRETIA BUCK	1875	Private nurse	" "
" ANNIE BRENNAN	1876	" "	" "
" MARY BRADY	1876	Now Mrs. Cummings, private nurse	" "
" MARY BESTOW	1876	At home	Coolville, Ohio.
" R. G. BELT	1878	Private nurse	New York City.

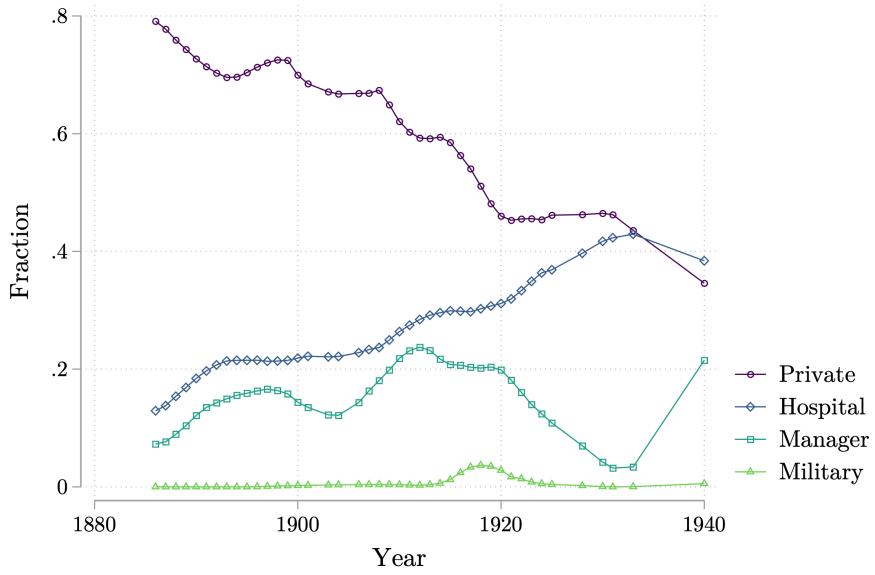
Source: Annual Report, Bellevue Hospital Training School for Nurses, 1887.

Figure A.11: *Fraction of graduates never married by birth cohort*



Notes: This figure shows the fraction of women measured as never married in training school alumnae records. The available data cover the 1860 to 1910 birth cohorts, where birth year is estimated using graduation year. The available time range is smaller for older age bins because the records end in 1940, which censors more recent cohorts. For details on the specific years and cohorts available, see Table A.4. Data have been smoothed with a bandwidth of 0.5.

Figure A.12: *Fraction of graduates employed in private, hospital, managerial, or military roles*



Notes: This figure shows the fraction of graduates employed in private, hospital, managerial, or military roles. Categories are not mutually exclusive. The sample is conditional on any listed employment among graduate nurses aged 20-39. For details on the specific years available, see Table A.4. Data have been smoothed with a bandwidth of 0.2.

Table A.4: *Statistics of nurse training schools with available records*

	Hospital founded	First nursing graduates	Mean cohort size	Total graduates	Observations
Bellevue[1]	1812	1875	33.5	1,249	12,166
Presbyterian[2]	1883	1906	54.7	1,478	8,899
MGH[3]	1811	1875	51.2	2,772	23,364
Total				5,499	44,429

Notes: This table reports statistics for Bellevue, Presbyterian, and MGH training schools for nurses.
[1]Data on Bellevue come from annual reports published starting in 1875. Graduating classes of 1875-1919 are compiled from reports published in 1886-93, 1895-1901, 1906-08, 1916-17, and 1919.
[2]Data on Presbyterian come from annual reports published starting in 1884. Graduating classes of 1906-32 are compiled from reports published in 1907-16, 1920-25, 1928, 1930, 1931, and 1933.
[3]Data on MGH come from annual reports published starting in 1888. Graduating classes of 1875-1939 are compiled from reports published in 1888-95, 1899, 1903-04, 1906-20, 1922, and 1940.

Table A.5: *Marriage and employment of graduates by age bin and decade*

Married				
	20-29	*30-39*	*40-49*	*50-59*
1890	0.13	0.27	.	.
1900	0.11	0.25	0.29	.
1910	0.18	0.32	0.32	0.30
1920	0.22	0.42	0.38	0.34
1930	0.28	0.58	0.62	.
1940	0.39	0.55	0.54	0.50
Employed | Currently married				
	20-29	*30-39*	*40-49*	*50-59*
1890	0.11	0.29	.	.
1900	0.05	0.06	0.23	.
1910	0.09	0.05	0.06	0.18
1920	0.03	0.03	0.06	0.02
1930	0.08	0.02	0.02	.
1940	0.38	0.24	0.16	0.17
Employed | Never married				
	20-29	*30-39*	*40-49*	*50-59*
1890	0.77	0.70	.	.
1900	0.65	0.58	0.41	.
1910	0.83	0.64	0.47	0.28
1920	0.69	0.66	0.54	0.32
1930	0.69	0.43	0.52	.
1940	0.86	0.78	0.68	0.49

Notes: This table reports marital status and employment conditional on marital status for women in each decade and age bin from 1890 to 1940. Married is an indicator for listing a married name. Widows and divorcees will be captured if the women reverts to her maiden name. Employed is an indicator for any occupation listed in the year, including military service. Some age-year cells are missing due to varying coverage of the training school records (Table A.4).

A.3 Data Appendix

A.3.1 Nurse training schools

Information on nurse training schools are contained in a variety of historical records. Soon after its founding in 1867, the U.S. Bureau of Education (the Bureau) began surveying educational institutions and training programs. Nurse training schools were educational in nature, therefore the Bureau took an early interest in them. I begin my data collection by digitizing school-level information contained in Bureau reports for the years 1879-1882 and 1884-1906. I digitize three

additional Bureau reports on nurse training schools to obtain data for 1911, 1918, and 1927 (Nutting, 1912; US Bureau of Education, 1919, 1928). I also digitize reports published by the American Nurses Association (ANA) in 1918 and 1922 (American Nurses Association, 1918, 1922). Available fields include city, year of organization, number of pupils, and school information such as program length and pay (Figures A.13 and A.14). Note that in some years, school-level statistics were not published, but the Bureau still collected data on total training schools, students, and graduates (see U.S. Bureau of the Census (1975), Series B 286-290). I add these data to the series in Figure 1.1.

Table A.6 provides summary statistics on training schools contained in the Bureau reports. The reports are largely similar across years, with a few exceptions. First, from 1898 onward, the Bureau listed schools affiliated with insane asylums separately. Between 5 and 10 percent of schools in a given year were affiliated with asylums. These schools had a similar training structure to schools affiliated with general hospitals. Second, in 1898, 1899, 1905, 1906, and 1911, the Bureau of Education identified post-graduate (supplemental) programs. These were typically 3 to 6 month programs meant for nurse graduates to supplement their training. Very few of these programs existed (1 to 3 percent of observations in a given year).

Schools for male nurses

The majority of nurse training programs from this era admitted exclusively white women, however, programs for (white) male nurses did exist. I identify schools admitting male nurses using reported pupils by gender (available starting in 1889). I flag schools that trained male nurses exclusively: these schools report no female pupils and can often be identified by name (e.g., City Hospital Training School for Male Nurses). In Table A.6, the fraction of schools with any male pupils falls over time from 17 percent in 1890 to 4 percent in 1927. Over 40 percent of schools with at least one male student were located at state psychiatric hospitals. About two to five programs in a given survey year were exclusively male. In 1927, the data contain one male-only training school.

Schools for black nurses

The Bureau and ANA reports did not record nurse pupils by race. Nursing in the early 20th century was racially segregated, and black nurses typically trained at black-founded and black-owned establishments (Hine, 1989). To better understand the numbers of black relative to white training schools, I turn to other sources. Hodson (1898), a manual for nurses, describes black-admitting and black-only schools. This source reports five schools for black nurses and six schools that admitted black probationers, but were not necessarily black-only. Multiple editions of *The Negro Yearbook*, published from 1912 to 1952, list hospitals and nursing schools for black individuals. The edition from 1912 lists 12 training schools for black women. Finally, Wesley (2010) gathered data on hundreds of historically black hospitals from numerous sources. The author notes that substantial effort was made to capture facilities that would have housed black trainee nurses. However, the information on founding date and presence of a nurse training school is incomplete.

Defining proximity to nurse training

To measure proximity to nurse training for women in the historical census, I first geocode all nurse training schools. School location is consistently reported as a state and town name (Figure A.13). I match school location to the Census Place Project, which contains consistently defined historical place names, latitude, and longitude (Berkes, Karger, and Nencka, 2023). I am able to match 95 percent of cities and towns directly. For the observations that do not match, I manually code the matched location to the nearest town.[3]

I count the number of nurse training schools in each location over time from 1870 to 1920. I construct separate counts using the Bureau and ANA reports, so that the two measures can be compared. To construct yearly counts from the Bureau reports, I first drop “post-graduate" schools, which were short supplemental courses for training school graduates and did not cater to new student nurses. These were listed separately from fully-fledged schools and make up

[3] This might occur if, for instance, a school location is listed as “Flatbush, NY", which I relabel as “New York, NY". In some cases, a school’s location will map to multiple places of the same name within a state (for instance, a town and an unincorporated place). I choose the place with the largest fraction of the population across census years, as defined in Berkes, Karger, and Nencka (2023). This affects 12 percent of observations in the training schools dataset.

less than 3 percent of observations in a given year. The Bureau recorded each school's year of organization up until 1905. I use this field in the data to measure the number of schools in each location and year from 1870 to 1904. After 1904, I count the number of schools recorded in each location and report year (1905, 1906, 1911, and 1918). To construct an analogous count using the ANA reports, I first drop data from the 1918 report, which contains substantial missingness in year of organization. Using the 1922 report, I drop "post-graduate" affiliations, which are listed separately. I then use year of organization to count the total number of schools in each location from 1870 to 1920.

For the remainder of the analysis, I use the count of schools based on the ANA's 1922 report since it contains better coverage of opening year than the Bureau reports. One drawback to relying on this source is that it only contains accredited schools as of 1922, not unaccredited schools or schools that have closed. The downside to relying on the Bureau reports alone is that, to my knowledge, 1905 is the last year that the Bureau of Education published the date of opening in its statistical reports. Additionally, I find evidence of a lag in reporting, especially in the earliest years of surveying when the Bureau was still in its infancy (Figure A.15, Panel (a)). For instance, the Freedmen's Hospital School of Nursing opened in 1894, but did not appear in the Bureau reports until 1896 (Coles, 1969). If a school is not surveyed until several years after opening, this approach will add significant measurement error. My approach is to use the ANA report as my primary source of openings, and to validate school counts over time using reports from the Bureau. In Figure A.15, Panel (b), I show that the two sources are well-aligned. The difference in 1918 may be partially due to the inclusion of post-graduate programs in the 1918 Bureau report.

With the geocoded locations of training schools over time, it is straightforward to measure distance to training school openings for women in the main analysis sample. Women are assigned a latitude and longitude from the Census Place Project, as discussed in the next section. Distance is calculated as a straight-line distance (in miles). While this measure is not the same as travel time, especially for long distances, the main analysis sample is restricted to women within ten miles of a training school. Note that only openings within the 1870-1920 period are considered. By starting the panel in 1870, I drop several training programs that were founded earlier. These programs were not based on the Nightingale model of training schools. I end the panel in 1920,

the last year that I can reliably measure school openings in the American Nurses Association (1922) report.

A.3.2 Census records

The complete-count census is enumerated every ten years and covers the entire U.S. population. I use the census files from 1870 to 1940, available via IPUMS (Ruggles et al., 2020). These records exclude the 1890 census, which was unable to be salvaged following a fire in 1921. For my analysis of the labor force after 1940, I add census samples from 1950 to 2015. Complete-count data for these years are preliminary (in the case of 1950) or not yet released. I use the following samples: 1 percent samples for 1950 and 1970; 5 percent samples for 1960, 1980, 1990, and 2000; American Community Survey for 2005, 2010, and 2015. Data are sourced from IPUMS (Ruggles et al., 2022).

The census contains demographic, household, and geographic characteristics, such as state and county. City identifiers are not universally available in IPUMS data, especially for individuals in smaller cities and towns. I link individuals in the complete-count census to the Census Place Project (Berkes, Karger, and Nencka, 2023), which provides a location identifier, latitude, and longitude for each individual. Locations are fine-grained, such as different suburbs outside of a major city. All individuals with a given location identifier receive the same latitude and longitude (there is no within-location variation). The match rate is approximately 95 percent.

Linked census samples

Linking individuals across census years is an area of substantial methodological research (Abramitzky et al., 2021). The latest innovation in this area is using data from the FamilySearch genealogical platform to link large samples of men and women over time (Buckles, Haws, et al., 2023). I use the FamilySearch data to construct two linked samples: a 1900-1920 linked sample and a 1900-1930 linked sample. One inherent drawback to this approach is that the set of linked individuals will differ in some respects from the entire U.S. population. For instance, black and foreign-born women are less likely to show up in the linked samples. I follow Bailey et al. (2020) and construct inverse propensity score weights that adjust for the likelihood of linkage. To link individuals from 1900 forward to 1920 and 1930, I take the following approach:

1. Append the linked sample to the complete 1900 census.
2. Construct a set of 1900 characteristics that may predict linkage, including:
    - Age in years
    - Length of first, middle, and last names
    - Indicator for any middle name
    - Commonness of last and first names, measured as the natural log of the number of people with that name
    - Number of siblings and indicator for any siblings
    - Indicators for race and gender
    - Length of father's first name and mother's first name
    - Indicators for state of birth
    - Indicator for foreign-born
    - Polynomials (quartic) in age, number of siblings, length of first/last name, length of father/mother name
    - Interaction of gender with race and first/last name commonness
    - Missingness indicators for first/last name, age, siblings, and father/mother first name
3. Estimate a probit with an indicator that takes on a value of 1 for the linked sample and 0 for the reference population (1900).
4. Calculate the inverse propensity score as: $\frac{(1-p)}{p} \times \frac{q}{(1-q)}$, where p is the propensity score, or the probability of being in the linked sample, and q is the linkage rate.

I plot the distribution of propensity scores for the reference population and linked samples in Figure A.16. While there is a substantial amount of overlap in the propensity score distributions, individuals with very low propensity scores (less than 0.2) may not be well-represented in the linked sample. In Tables A.8 and A.9, I examine the characteristics of the sample of white women born in the U.S. between 1885 and 1900. The linked samples are somewhat selected on characteristics that predict linkage (such as having more siblings or a middle initial). Reweighting the linked sample in column (3) aligns these observables more closely with the full sample.

Defining the labor force

I frequently identify individuals as “in the labor force". The measurement of employment and occupation has changed over time, therefore the definition of the labor force differs across census years. I adopt a definition of the labor force that is in line with prior literature (Katz and Margo, 2014). I restrict to individuals aged 15 to 64 with a valid occupation code (*occ1950* between 0 and 970, dropping armed forces code 595). For years 1870, 1880, 1900, and 1920, I restrict to individuals in the labor force (*labforce* = 2). For 1910 and 1930, I restrict to employed individuals (*empstat* = 1). For 1940 onward, I restrict to employed individuals (*empstat* = 1) and drop members of the armed forces (drop if *empstatd* = 13, 14, or 15). To ensure the 1950-2015 samples are representative, I apply person weights (*perwt*). However, these weights do not account for the intensive margin of total labor supplied (Autor and Dorn, 2013). I therefore report additional results weighted by the product of person weights and total labor supplied during the year (weeks worked multiplied by usual hours per week).

Measuring occupation and economic status

The census began collecting income data in 1940. For the pre-1940 period, prior literature relies on occupation to examine trends in economic status and intergenerational mobility. Occupation can be mapped to economic status in a number of ways, some of which I discuss here. A common approach is to measure economic status using the variable *occscore*, constructed by IPUMS as the median income level of an occupation in 1950 (Ruggles et al., 2022; Olivetti and Paserman, 2015). This variable will not capture changes in occupational earnings over time before 1950 (for instance, if incomes in healthcare grew relatively quickly from 1900 to 1950, then *occscore* will overstate economic standing in 1900). This is a fundamental issue that prior literature has tried to address. In particular, Song et al. (2020) use an occupation ranking procedure that weights by relative education level within each birth cohort and assigns occupations to a percentile rank (0-100). This measure is meant to capture the relative economic status of individuals in a given occupation over time. Ward (2023) constructs an “adjusted Song score" that allows occupational rank to vary by race and region. Buckles, Price, et al. (2023) partition the Song score by race, region, and gender.

For my analysis of economic status, I start with the standard *occscore* assigned to each occupation. I use a modified version of the *occscore*, the lasso-adjusted industry, demographic, and occupation (LIDO) scores (Saavedra and Twinam, 2020). This version allows occupation score to vary more flexibly, but does not solve the fundamental challenge of mapping economic status back in time. I follow Buckles, Price, et al. (2023) and estimate adjusted Song occupation scores, stratifying by cohort, region, gender, and race. While adjusted Song scores are relative and not absolute measures, they capture shifts in each occupation's human capital content prior to 1950. To avoid the challenge of shifting occupational categories, these scores are defined over "microclass" occupational groupings (for instance, nurses fall under "health semi-professionals", see Song et al. (2020)). In practice, I show results separately for each of these different measures.

Figure A.18 plots the distribution of adjusted Song scores for women in each of the four census regions. The scores are shown for white women born from 1895 to 1904. While some variation exists across regions, nurses score relatively lower than clerks and teachers, and relatively higher than housekeepers. Absolute measures of economic status, such as *occscore*, show a similar pattern. The *occscore* for nurses (in hundreds of 1950 dollars) is 21, compared to 27 for teachers and 25 for clerical workers. Similarly, the LIDO scores for white women, aggregated over industry, state, and age, are 15 for nurses, 16 for teachers, and 18 for clerical workers (Figure A.17).

A.3.3 Additional data sources

Licensing and certification laws

State laws regulating registered nurses and licensed practical nurses are sourced from Carollo (2024). The data include all state statutes passed since the late 1800s concerning the registration, certification, and licensing of nurses. State laws regulating midwives are sourced from Anderson et al. (2020). States are coded as having a law in the first year that the law went into effect, which sometimes differs from the year of enactment. See Carollo (2024) and Anderson et al. (2020) for more details on the historical sources of these laws.

Figure A.13: *Excerpt from the 1905 Report of the Commissioner of Education*

TABLE 3.—*Statistics of training schools for nurses, for the year 1904–5.*

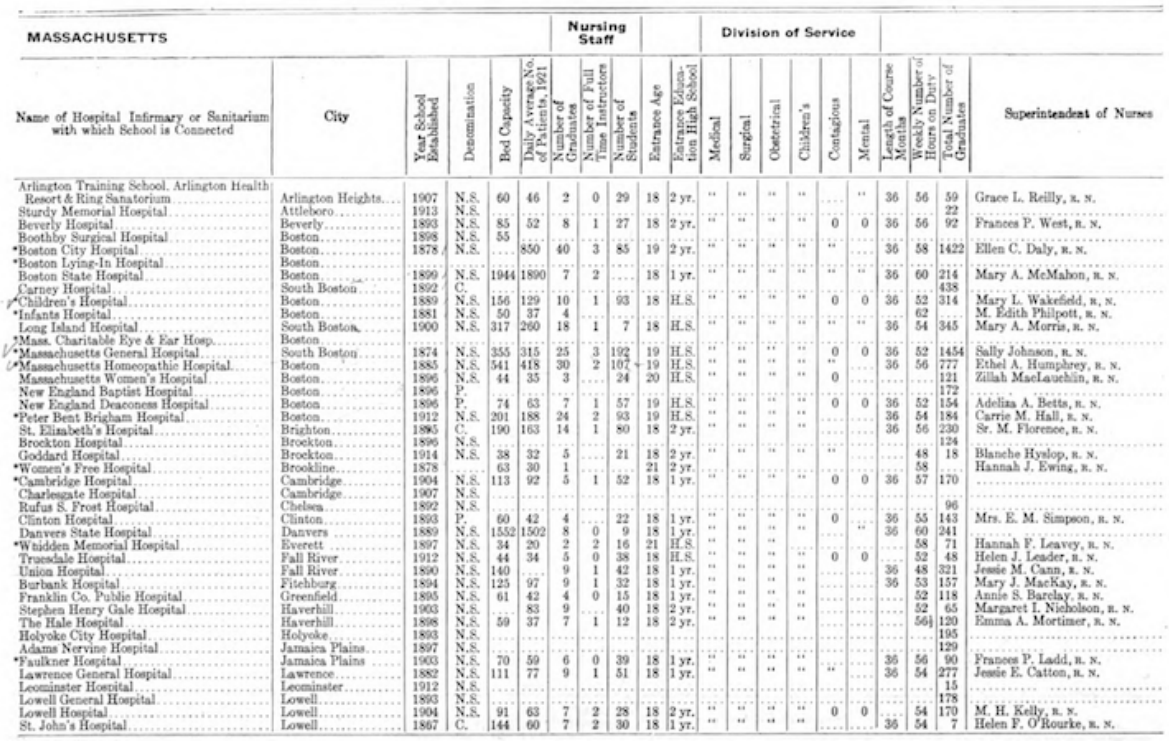
	Location.	Name of institution.	Capacity of hospital (beds).	Year nurse school first opened.	Superintendent of nurses.	Session closes (about).	Nurse pupils.			Years in the course.	Are pupils sent to nurse out-patients.	Pupils receive free board, (l) laundry, (u) uniforms, (t) text-books.	Monthly allowance to pupils.			Estimated value of grounds and buildings of hospital.
							Women.	Men.	Graduated in 1905.				First year.	Second year.	Third year.	
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
	ALABAMA.															
1	Birmingham	Hillman Hospital	80	1903	Jane W. Barry	Feb. 28	11	....	7	3	No	b, l	$7	$7	$9	$100,000
2	do	St. Vincent's Hospital	150	1900	Sister Chrysostom	June 12	35	....	12	3	Yes	b, l	5	5	5	200,000
3	Mobile	Providence Infirmary	50	1902	Sister Agatha	June 1	19	....	6	3	Yes	b, l	5	5	5	150,000
4	Montgomery	St. Margaret's Hospital	75	1903	Sister Margaret	Jan. —	14	....	0	3	Yes	b, l	.....	.....	.....	150,000
5	Normal	M. French Sheldon Hospital of Agricultural and Mechanical College.	.....	1895	Mrs. F. M. Innis	May 25	36	0	7	2	Yes	.....	.....	.....	.....	900
6	Tuskegee Institute	Tuskegee Institute Hospital	25	1892	John A. Kenney	do	18	6	5	3	Yes	b, l	0	0	0	2,000
	ARKANSAS.															
7	Hot Springs	St. Joseph's Infirmary	100	1905	—— Revelle	.....	5	....	....	2	.....	b, l	5	5	.....	.....
8	Little Rock	Pulaski County Hospital	200	1902	Kate M. Illing	Apr. 20	14	0	7	3	Yes	b, l	8	10	10	75,000
	CALIFORNIA.															
9	Eureka	Sequoia Hospital and Sanitarium.	30	1903	D. L. Anderson	Dec. 29	9	0	4	3	Yes	b	7	7	7	30,000
10	Los Angeles	California Hospital	200	1889	Helen W. Kelly	June 5	70	....	26	3	No	b	6	9	12	300,000
11	do	County Hospital	350	1895	N. Estelle Woods	June 1	25	4	11	2	No	b, l	5	10	.....	.....
12	do	Deaconess Hospital*	40	.....	Anna Du Sold	.....	30	1	....	3	.....	.....	8	10	12	90,000
13	do	Emergency and General Hospital.	150	1903	Nora E. Brown	(a)	35	3	20	3	Yes	b, l	6	6	8	70,000
14	do	Good Samaritan Hospital*	100	1897	Harriet W. Pahl	June —	37	0	10	3	.....	.....	5	8	10	150,000
15	do	Los Angeles Hospital	12	1900	Lydia Wismar	.....	4	....	4	2	No	b	10	12	.....	.....
16	do	Los Angeles Infirmary	200	1900	Sister Regis	June 15	60	2	19	3	Yes	b, l	5	5	5	.....
17	Oakland	Fabiola Hospital	100	1887	Katharine Fitch	June —	59	4	12	3	Yes	b, l	9	9	9	110,000
18	do	Providence Hospital	100	1904	Sister Mary Alphonse	Apr. 5	30	0	0	3	Yes	b, l	5	5	5	.....

*In 1903–4.

a No definite session.

Source: US Bureau of Education (1907).

Figure A.14: *Excerpt from the 1922 Report of the American Nurses Association*



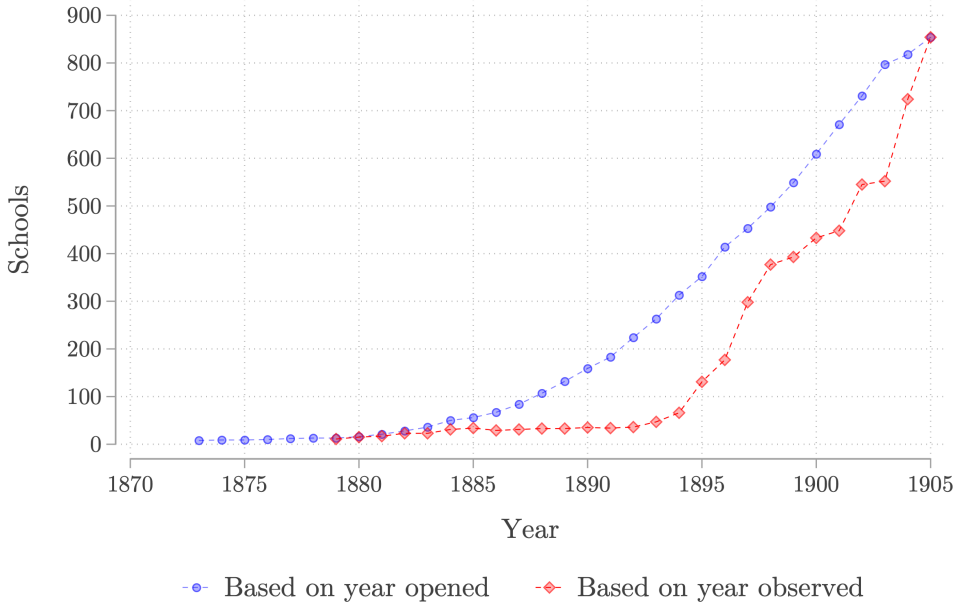
MASSACHUSETTS

Name of Hospital Infirmary or Sanitarium with which School is Connected	City	Year School Established	Denomination	Bed Capacity	Daily Average No. of Patients, 1921	Nursing Staff: Number of Graduates	Nursing Staff: Number of Full Time Instructors	Nursing Staff: Number of Students	Entrance Age	Entrance Education High School	Division of Service: Medical	Surgical	Obstetrical	Children's	Contagious	Mental	Length of Course Months	Weekly Number of Hours on Duty	Total Number of Graduates	Superintendent of Nurses
Arlington Training School, Arlington Health Resort & Ring Sanatorium	Arlington Heights	1907	N.S.	60	46	2	0	29	18	2 yr.	"	"	"	"	...	"	36	56	59	Grace L. Reilly, R. N.
Sturdy Memorial Hospital	Attleboro	1913	N.S.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	22	...
Beverly Hospital	Beverly	1893	N.S.	85	52	8	1	27	18	2 yr.	"	"	"	"	0	0	36	56	92	Frances P. West, R. N.
Boothby Surgical Hospital	Boston	1898	N.S.	55	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
*Boston City Hospital	Boston	1878	N.S.	...	850	40	3	85	19	2 yr.	"	"	"	"	"	...	36	58	1422	Ellen C. Daly, R. N.
*Boston Lying-In Hospital	Boston	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Boston State Hospital	Boston	1899	N.S.	1944	1890	7	2	...	18	1 yr.	"	"	"	"	"	"	36	60	214	Mary A. McMahon, R. N.
Carney Hospital	South Boston	1892	C.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	438	...
*Children's Hospital	Boston	1889	N.S.	156	129	10	1	93	18	H.S.	"	"	"	"	0	0	36	52	314	Mary L. Wakefield, R. N.
*Infants Hospital	Boston	1881	N.S.	50	37	4	...	...	...	...	...	...	...	...	...	...	...	62	...	M. Edith Philpott, R. N.
Long Island Hospital	South Boston	1900	N.S.	317	260	18	1	7	18	H.S.	"	"	"	"	"	"	36	54	345	Mary A. Morris, R. N.
*Mass. Charitable Eye & Ear Hosp.	Boston	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
*Massachusetts General Hospital	South Boston	1874	N.S.	355	315	25	3	192	19	H.S.	"	"	"	"	0	0	36	52	1454	Sally Johnson, R. N.
*Massachusetts Homeopathic Hospital	Boston	1885	N.S.	541	418	30	2	107	19	H.S.	"	"	"	"	"	...	36	56	777	Ethel A. Humphrey, R. N.
Massachusetts Women's Hospital	Boston	1896	N.S.	44	35	3	...	24	20	H.S.	"	"	"	"	0	...	...	...	121	Zillah MacLauchlin, R. N.
New England Baptist Hospital	Boston	1896	P.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	172	...
New England Deaconess Hospital	Boston	1896	P.	74	63	7	1	57	19	H.S.	"	"	"	"	0	0	36	52	154	Adeliza A. Betts, R. N.
*Peter Bent Brigham Hospital	Boston	1912	N.S.	201	188	24	2	93	19	H.S.	"	"	"	"	...	...	36	54	184	Carrie M. Hall, R. N.
St. Elizabeth's Hospital	Brighton	1895	C.	190	163	14	1	80	18	2 yr.	"	"	"	"	...	...	36	56	230	Sr. M. Florence, R. N.
Brockton Hospital	Brockton	1896	N.S.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	124	...
Goddard Hospital	Brockton	1914	N.S.	38	32	5	...	21	18	2 yr.	"	"	"	"	"	...	...	48	18	Blanche Hyslop, R. N.
*Women's Free Hospital	Brookline	1878	...	63	30	1	...	...	21	2 yr.	...	...	...	...	...	...	...	58	...	Hannah J. Ewing, R. N.
*Cambridge Hospital	Cambridge	1904	N.S.	113	92	5	1	52	18	1 yr.	"	"	"	"	0	0	36	57	170	...
Charlesgate Hospital	Cambridge	1907	N.S.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Rufus S. Frost Hospital	Chelsea	1892	N.S.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	96	...
Clinton Hospital	Clinton	1893	P.	60	42	4	...	22	18	1 yr.	"	"	"	"	0	...	36	55	143	Mrs. E. M. Simpson, R. N.
Danvers State Hospital	Danvers	1889	N.S.	1552	1502	8	0	9	18	1 yr.	"	"	"	"	...	"	36	60	241	...
*Whidden Memorial Hospital	Everett	1897	N.S.	34	20	2	2	16	21	H.S.	"	"	"	...	...	...	...	58	71	Hannah F. Leavey, R. N.
Truesdale Hospital	Fall River	1912	N.S.	44	34	5	0	38	18	H.S.	"	"	"	"	0	0	...	52	48	Helen J. Leader, R. N.
Union Hospital	Fall River	1890	N.S.	140	...	9	1	42	18	1 yr.	"	"	"	"	...	...	36	48	321	Jessie M. Cann, R. N.
Burbank Hospital	Fitchburg	1894	N.S.	125	97	9	1	32	18	1 yr.	"	"	"	"	...	...	36	53	157	Mary J. MacKay, R. N.
Franklin Co. Public Hospital	Greenfield	1895	N.S.	61	42	4	0	15	18	1 yr.	"	"	"	"	...	...	...	52	118	Annie S. Barclay, R. N.
Stephen Henry Gale Hospital	Haverhill	1903	N.S.	...	83	9	...	40	18	2 yr.	"	"	"	"	...	...	...	52	65	Margaret I. Nicholson, R. N.
The Hale Hospital	Haverhill	1898	N.S.	59	37	7	1	12	18	2 yr.	"	"	"	"	...	...	...	56	120	Emma A. Mortimer, R. N.
Holyoke City Hospital	Holyoke	1893	N.S.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	195	...
Adams Nervine Hospital	Jamaica Plains	1897	N.S.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	129	...
*Faulkner Hospital	Jamaica Plains	1903	N.S.	70	59	6	0	39	18	1 yr.	"	"	"	"	"	...	36	56	90	Frances P. Ladd, R. N.
Lawrence General Hospital	Lawrence	1882	N.S.	111	77	9	1	51	18	1 yr.	"	"	"	"	"	...	36	54	277	Jessie E. Catton, R. N.
Leominster Hospital	Leominster	1912	N.S.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	15	...
Lowell General Hospital	Lowell	1893	N.S.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	178	...
Lowell Hospital	Lowell	1904	N.S.	91	63	7	2	28	18	2 yr.	"	"	"	"	0	0	...	54	170	M. H. Kelly, R. N.
St. John's Hospital	Lowell	1867	C.	144	60	7	2	30	18	1 yr.	"	"	"	"	...	...	36	54	7	Helen F. O'Rourke, R. N.

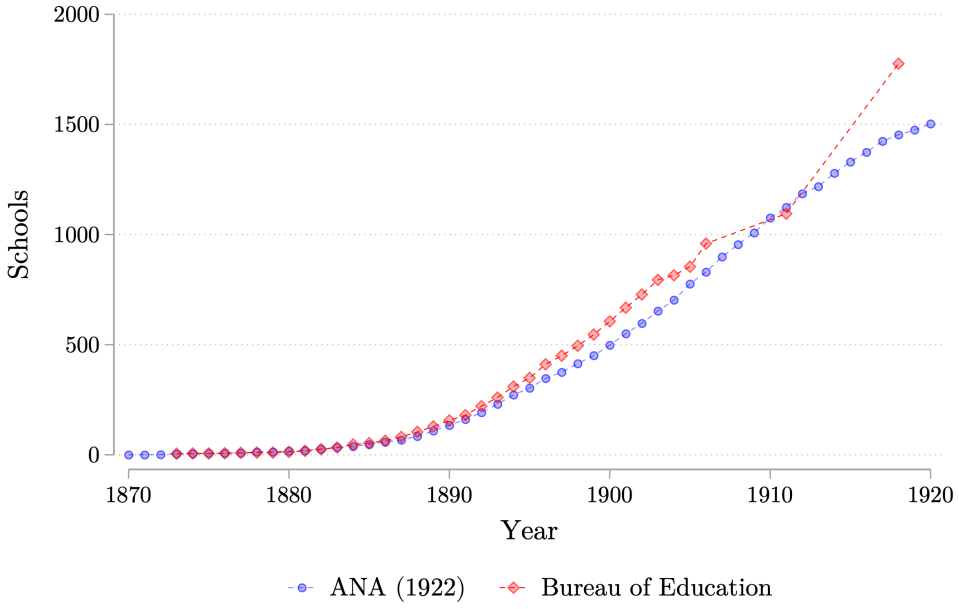
Source: American Nurses Association (1922)

Figure A.15: *Count of training schools over time from different sources*

(a) *Bureau reports*



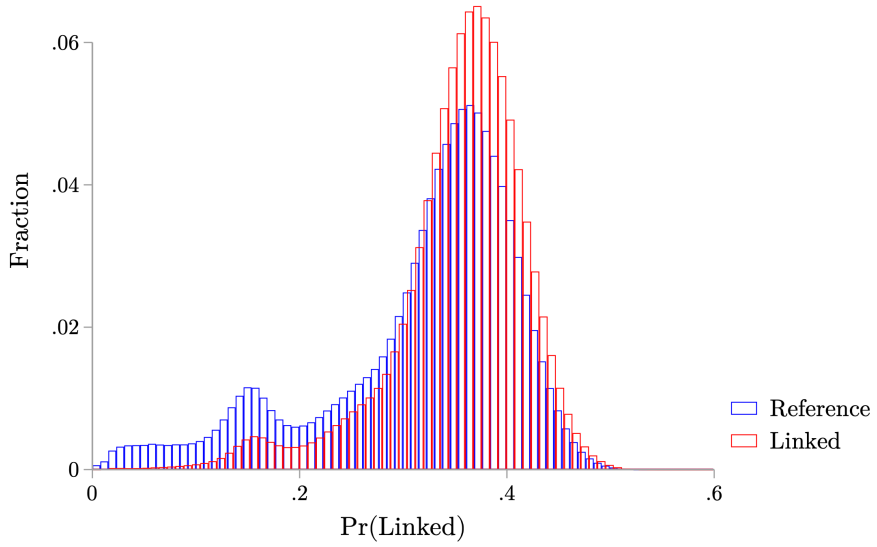
(b) *Comparison of ANA and Bureau reports*



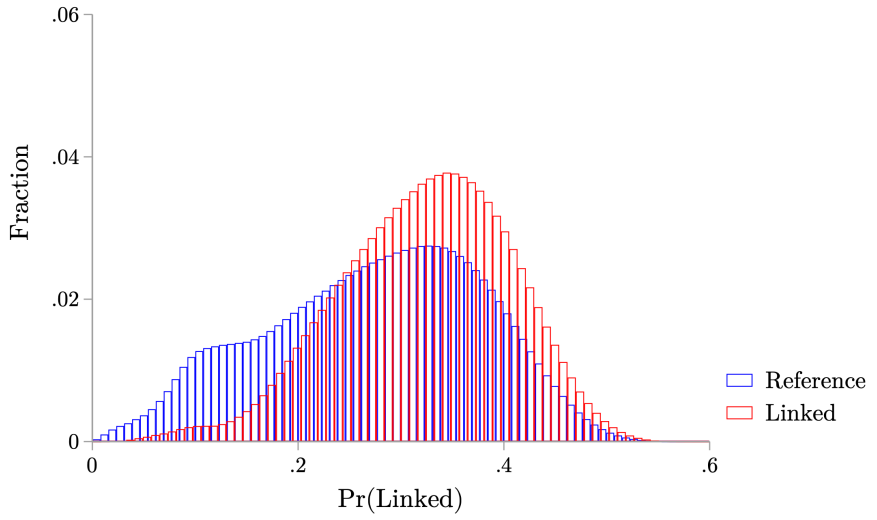
Notes: Figure reports the count of nurse training schools over time from several sources described in Appendix Section A.3.1. Panel (a) reports counts constructed from reports of the U.S. Bureau of Education, 1879-1905. Counts are shown based on year opened and year observed. Panel (b) compares counts in American Nurses Association (1922) and the Bureau reports from 1870 to 1920.

Figure A.16: *Propensity scores, 1900 reference population*

(a) *1900-1920 linkage*

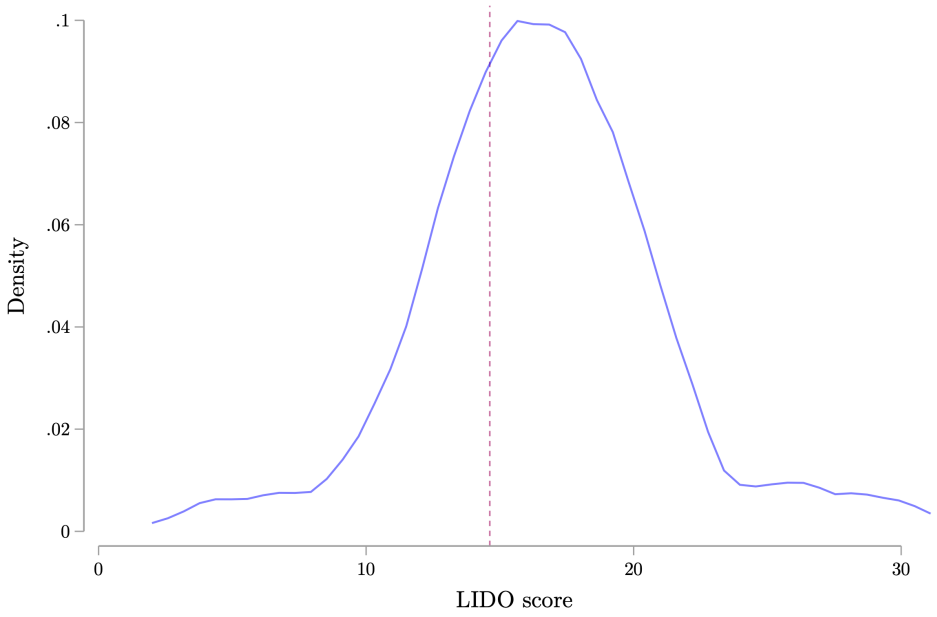


(b) *1900-1930 linkage*



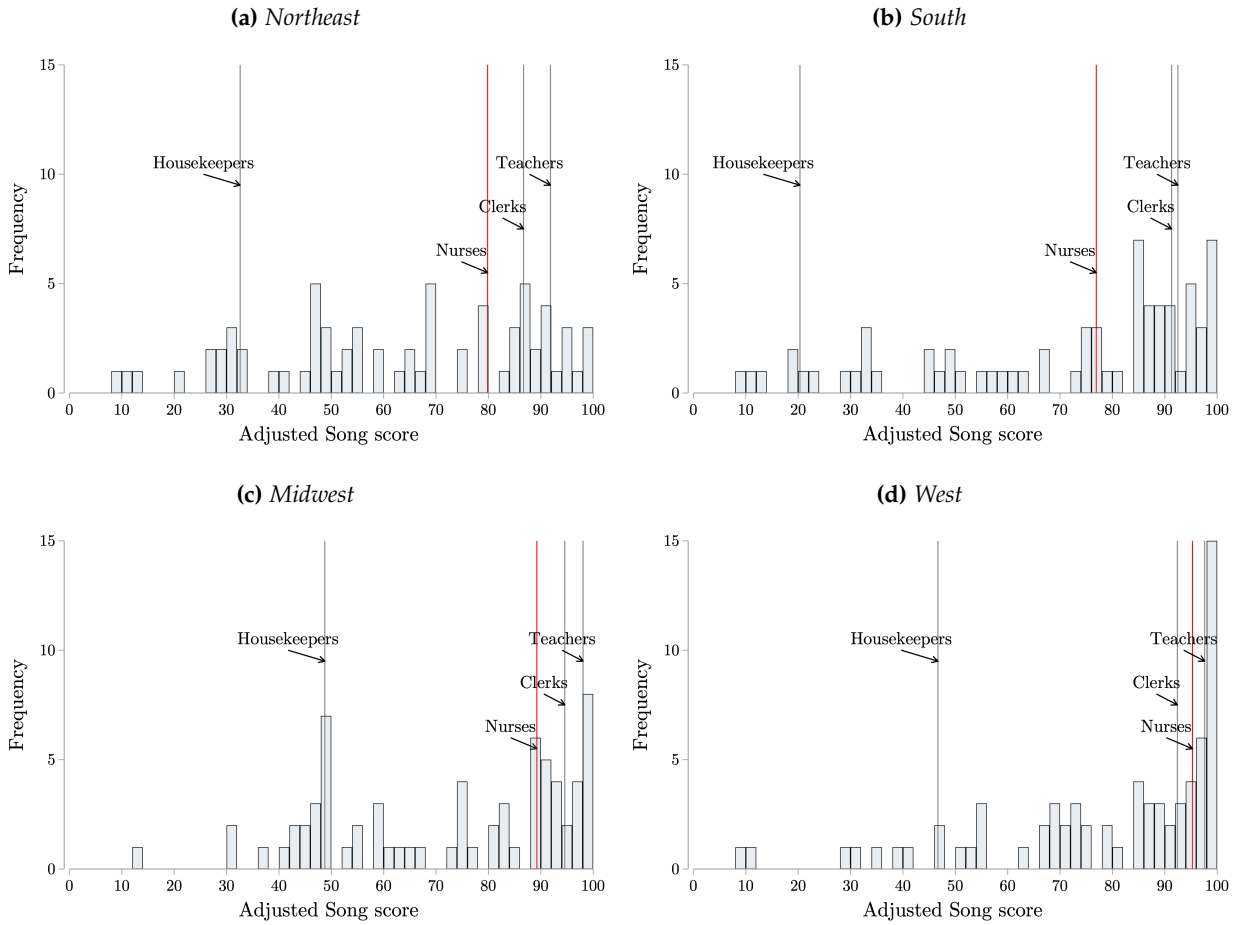
Notes: Figure reports propensity score distributions constructed according to Appendix Section A.3.2. In Panel (a), propensity scores are shown for the 1900-1920 census linkage. Plotted in blue are propensity scores for the complete-count 1900 census (the reference population). Plotted in red are propensity scores for the sample linked to 1920. Panel (b) reports analogous propensity scores for the 1900-1930 census linkage.

Figure A.17: *LIDO scores, white women at age 25*



Notes: Figure reports the distribution of LIDO income scores for white women at age 25. LIDO score is constructed from the 1950 census (Saavedra and Twinam, 2020). The red vertical line indicates the LIDO score for professional nurses.

Figure A.18: *Adjusted Song scores for white women born 1895-1904*



Notes: Figure reports histograms of adjusted Song scores for white women born from 1895 to 1904. Each panel (a)-(d) is a census region. Observations are micro-class occupation groups following Song et al. (2020). Vertical lines denote the locations of the micro-class groups containing nurses, clerks, teachers, and housekeepers. The line for nurses is shown in red. See Appendix Section A.3.2 for details.

Table A.6: *Statistics of nurse training schools from the Bureau of Education*

Year	(1) Schools	(2) Students	(3) Graduates	(4) Average length	(5) Average students	(6) Percent any men	(7) Percent asylum	(8) Percent postgrad
1879	11	298	141	1.7	27.1	.	.	.
1880	15	323	157	1.7	24.8	.	.	.
1881	17	414	133	1.8	25.9	.	.	.
1882	23	475	124	1.9	25.0	.	.	.
1884	31	579	221	1.8	23.2	.	.	.
1885	34	793	218	1.8	24.8	.	.	.
1886	29	837	349	1.8	28.9	.	.	.
1887	31	989	335	1.9	31.9	.	.	.
1888	33	1,093	421	2.0	34.2	.	.	.
1889	33	1,255	443	1.9	38.0	9.1	.	.
1890	35	1,552	471	1.9	44.3	17.1	.	.
1891	34	1,613	527	1.9	47.4	14.7	.	.
1892	36	1,862	582	1.9	51.7	16.7	.	.
1893	47	2,338	786	2.0	50.8	15.2	.	.
1894	66	2,710	970	2.0	41.7	15.4	.	.
1895	131	3,994	1,489	2.0	31.0	15.5	.	.
1896	177	5,093	1,773	2.1	28.8	15.8	.	.
1897	298	7,254	2,498	2.1	24.3	17.1	.	.
1898	388	9,072	3,223	2.2	23.4	19.1	7.0	2.8
1899	405	10,387	3,328	2.2	25.6	17.3	8.9	3.0
1900	433	11,164	3,539	2.3	25.8	17.6	9.7	.
1901	448	11,599	3,712	2.4	25.9	17.4	10.0	.
1902	545	13,231	3,998	2.4	24.3	17.1	9.7	.
1903	552	13,788	4,201	2.5	25.0	16.7	9.8	.
1904	724	17,364	5,232	2.5	24.0	17.5	7.7	.
1905	862	20,280	5,805	2.6	23.6	15.1	6.7	0.9
1906	974	22,831	6,574	2.6	23.5	14.8	7.0	1.5
1911	1,118	29,685	7,707	2.7	26.9	.	6.3	2.1
1918	1,776	55,374	13,791	2.9	32.8	6.1	4.7	.
1927	1,797	77,767	18,628	2.9	43.3	3.8	4.2	.

Sources: U.S. Bureau of Education, 1879-1882, 1884-1906, 1911, 1918, 1927. See Appendix Section A.3.1.
Notes: Table reports descriptive statistics for nurse training schools in each year that data is available from the U.S. Bureau of Education. Columns (1), (2), and (3) report the total schools, students, and graduates. Columns (4) and (5) report the average years of study and the average number of students. Columns (6), (7), and (8) report the percent of schools with any male students, the percent affiliated with an asylum, and the percent classified as postgraduate schools. Percents are missing in years where student breakdown by gender, asylum status, and postgraduate status are not reported.

Table A.7: *Occupation codes for nurses in the census*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
								Census year								
Job Title	1870	1880	1900	1910	1920	1930	1940	1950	1960	1970	1980	1990	2000	2005	2010	2015
Professional nurse	46[1,2,4]	46[1,2,4]	46[1,2,4]	872[1,2]	872[1,2]	216[1,2]	19[1]	58[1]	150[1]	75[1]	95[1]	95[1]	313[1]	3130[1]	3255[1], 3256[1], 3258[1]	3255[1], 3256[1], 3258[1]
Student nurse							19[1]	59[2]	151[2]	923[2]						
Midwife	44[3]	44[3]	44[3]	934[3]	934[3]	257[3]	760[4]	772[3]	840[3]	924[3]						
Practical nurse				936[4]	936[4]	258[4]	760[4]	781[4]	842[4]	926[4]	207[4]	207[4]	350[4]	3500[4]	3500[4]	3500[4]
Health administrator										212[1]	15[1]	15[1]	35[1]	350[1]	350[1]	350[1]
Physician assistant											106[1]	106[1]	311[1]	3110[1]	3110[1]	3110[1]

Notes: Table reports census occupation codes for nurses from 1870 to 2015. Titles include professional nurse (e.g., trained nurse or registered nurse), student nurse, midwife, practical nurse (e.g., licensed practical or vocational nurse), health administrator (e.g., medical and health services manager), and physician assistant. Blank cells indicate that the job title did not receive a code in that census year's classification. Superscripts indicate the occupation codes that are included in the 1950 classification of (1) professional nurse (*occ1950* = 58), (2) student nurse (*occ1950* = 59), (3) midwife (*occ1950* = 772), and (4) practical nurse (*occ1950* = 781). The 1870-1900 occupation code for professional nurse likely captures all nurses except midwives. Professional and student nurses were combined under one category in 1940, as were midwives and practical nurses. Prior to 1940, practical nurses were classified under the title "untrained nurse". All codes are sourced from IPUMS (Ruggles et al., 2022).

Table A.8: *Descriptive statistics, 1900-1920 linked sample*

	(1) 1900 sample	(2) Linked 1920	(3) Linked 1920
Age	7.15	6.86	7.13
Number of siblings	3.07	3.17	3.16
Region = North	0.27	0.25	0.26
Region = Midwest	0.40	0.43	0.41
Region = South	0.28	0.28	0.28
Region = West	0.05	0.05	0.05
Length of first name	5.36	5.38	5.36
Length of last name	6.35	6.36	6.37
Middle initial	0.37	0.41	0.38
First name commonness	10.80	10.85	10.74
Last name commonness	7.81	7.99	7.78
Father present	0.90	0.94	0.92
Father age	40.30	40.08	40.25
Father literate	0.92	0.93	0.93
Father employed	0.91	0.92	0.92
Mother present	0.94	0.96	0.96
Mother age	35.56	35.39	35.60
Mother literate	0.91	0.93	0.93
Mother employed	0.05	0.04	0.04
Reweighted	No	No	Yes
Observations	11,420,519	6,331,497	6,331,497

Notes: Table reports descriptive statistics for the 1900-1920 linked sample. Column (1) reports means for the population of white women born in the U.S. between 1885 and 1900 and observed in the 1900 census. Column (2) reports means for the subset that can be linked to the 1920 census via FamilySearch. Column (3) reports means for the linked subset in column (2), reweighted using the inverse propensity score reweighting approach described in Appendix Section A.3.2.

Table A.9: *Descriptive statistics, 1900-1930 linked sample*

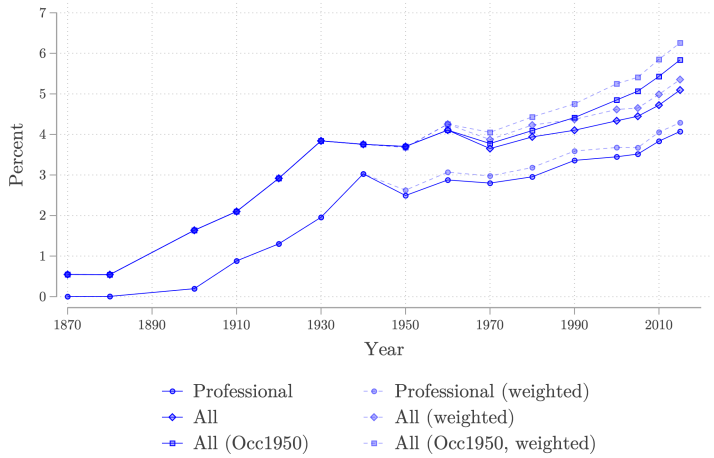
	(1)	(2)	(3)
	1900 sample	Linked 1930	Linked 1930
Age	7.15	7.07	7.52
Number of siblings	3.07	3.21	3.23
Region = North	0.27	0.23	0.25
Region = Midwest	0.40	0.44	0.41
Region = South	0.28	0.29	0.29
Region = West	0.05	0.05	0.05
Length of first name	5.36	5.36	5.35
Length of last name	6.35	6.35	6.36
Middle initial	0.37	0.42	0.38
First name commonness	10.80	10.82	10.68
Last name commonness	7.81	8.00	7.79
Father present	0.90	0.94	0.94
Father age	40.30	40.28	40.63
Father literate	0.92	0.93	0.93
Father employed	0.91	0.92	0.92
Mother present	0.94	0.96	0.97
Mother age	35.56	35.53	35.92
Mother literate	0.91	0.93	0.93
Mother employed	0.05	0.04	0.04
Reweighted	No	No	Yes
Observations	11,420,519	5,533,161	5,533,161

Notes: Table reports descriptive statistics for the 1900-1930 linked sample. Column (1) reports means for the population of white women born in the U.S. between 1885 and 1900 and observed in the 1900 census. Column (2) reports means for the subset that can be linked to the 1930 census via FamilySearch. Column (3) reports means for the linked subset in column (2), reweighted using the inverse propensity score reweighting approach described in Appendix Section A.3.2.

A.4 Supplementary Tables and Figures

Figure A.19: *Percent of the labor force in nursing by gender*

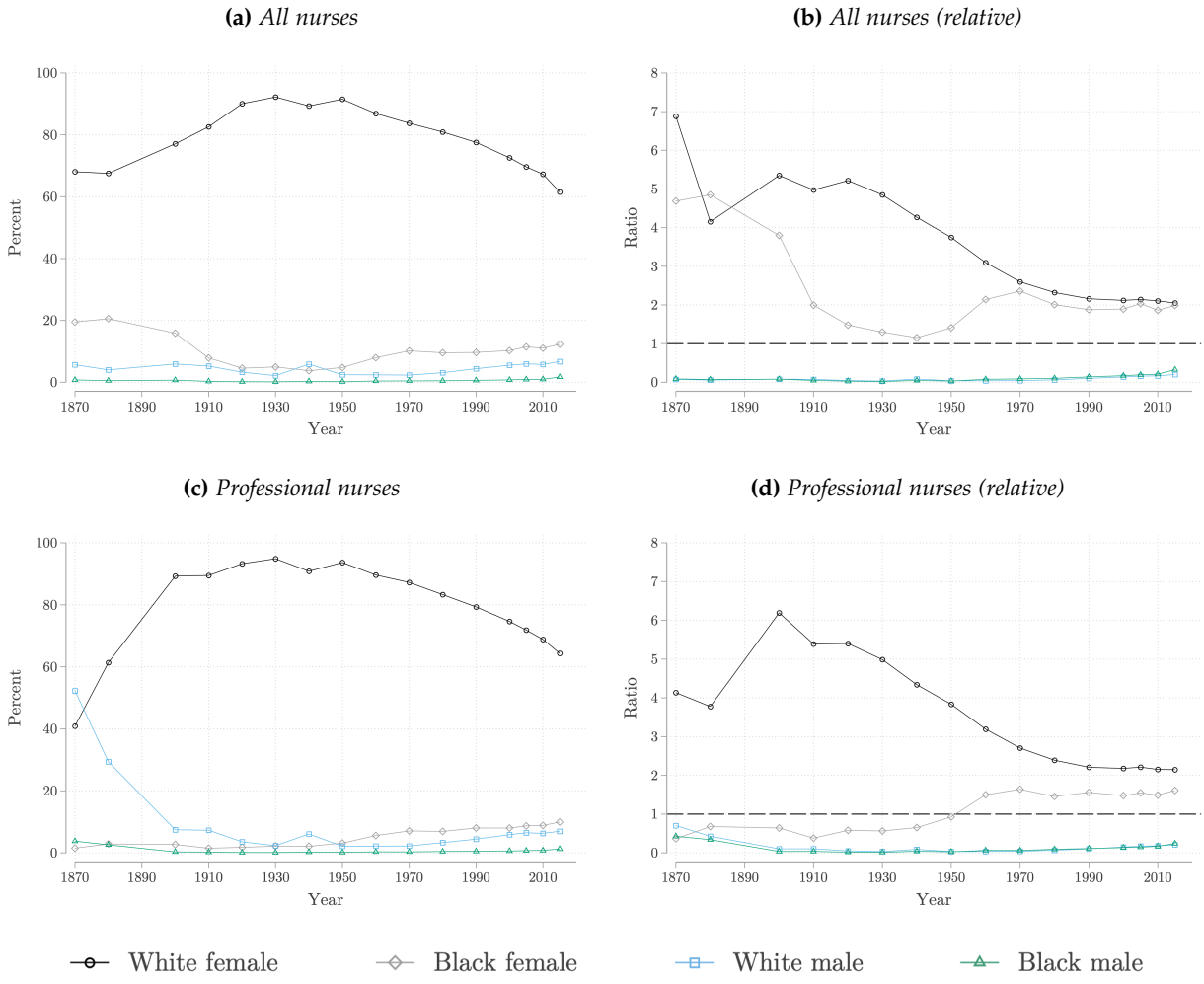
(a) *Women*



(b) *Men*

Notes: Figure reports the percent of the labor force in nursing by gender. The labor force is defined in Appendix Section A.3.2. Prior to 1950, the "Professional" series includes individuals with a 1950 occupation code of 58. For 1950 onward, the series includes professional nurses defined according to the codes in Table A.7. The "All" series includes professional, student, and untrained (practical) nurses defined according to the codes in Table A.7. The "All (Occ1950)" series counts all nurses according to the 1950 classification. Data come from the complete-count census for 1870-1940; 1% samples for 1950 and 1970; 5% samples for 1960, 1980, 1990, and 2000; American Community Survey for 2005, 2010, and 2015. The "Professional", "All", and "All (Occ1950)" series use person weights for the 1950-2015 samples. The "Professional (weighted)", "All (weighted)", and "All (Occ1950, weighted)" series use labor supply weights for the 1950-2015 samples.

Figure A.20: *Demographic composition of nursing*



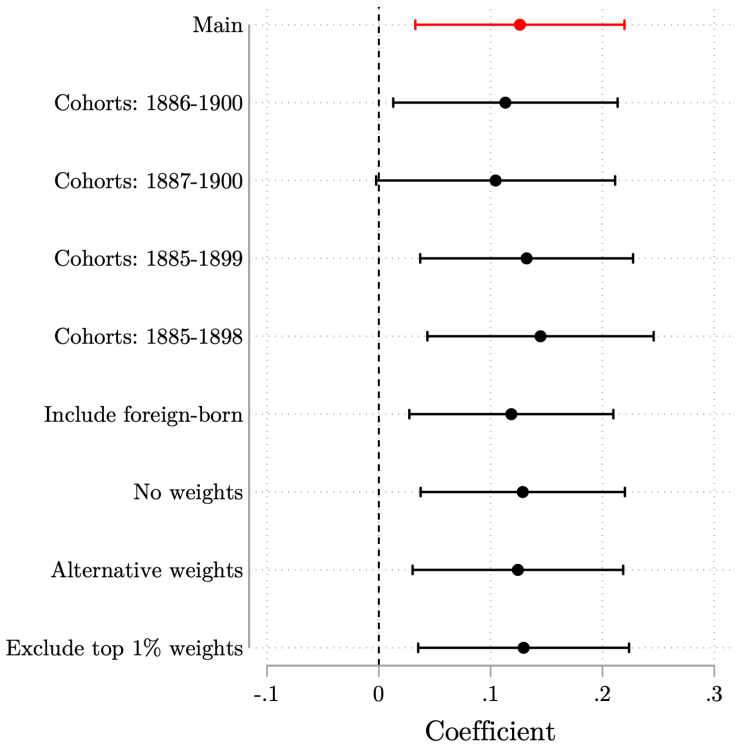
Notes: Figure shows the demographic composition of nurses from 1870 to 2015. Panel (a) reports the percent of all nurses that are white female, black female, white male, and black male. Panel (b) transforms the percents in Panel (a) into ratios that represent the disproportionate representation of each group relative to the overall labor force (for instance, percent of all nurses who are black female divided by percent of the labor force that is black female). The labor force is defined in Appendix Section A.3.2. Panels (c) and (d) report the same statistics for professional nurses. Prior to 1950, "professional nurses" are individuals with a 1950 occupation code of 58. For 1950 onward, professional nurses are defined according to the codes in Table A.7. Data come from the complete-count census for 1870-1940; 1% samples for 1950 and 1970; 5% samples for 1960, 1980, 1990, and 2000; American Community Survey for 2005, 2010, and 2015. Person weights are used for the 1950-2015 samples.

Table A.10: *Training schools and county nurse supply*

	(1)	(2)	(3)
Panel A: Log number of trained nurses			
Mean at s = -10		0.41	
Linear pretrend	[-0.24]	[-0.20]	[-0.17]
Coefficients (detrended):			
Years 0 to 10	0.80	0.81	0.73
	(0.06)	(0.06)	(0.06)
Years 10 to 20	1.54	1.55	1.37
	(0.09)	(0.09)	(0.09)
Years 20+	1.91	1.94	1.62
	(0.12)	(0.12)	(0.13)
Controls		T	T, S-Y
Weights	No	No	No
Panel B: Rate of trained nurses per 1,000			
Mean at s = -10		2.71	
Linear pretrend	[-0.85]	[-0.88]	[-1.28]
Coefficients (detrended):			
Years 0 to 10	4.73	4.95	4.63
	(0.51)	(0.53)	(0.52)
Years 10 to 20	10.58	11.13	10.56
	(1.00)	(1.04)	(1.08)
Years 20+	12.68	13.45	12.32
	(1.71)	(1.76)	(1.95)
Controls		T	T, S-Y
Weights	Yes	Yes	Yes

Notes: Table reports event study estimates of equation (1.1). Panel A reports results using the log number of trained nurses among women ages 15-24. Panel B reports results using the rate of trained nurses per 1,000 women. See Section 1.2.4 for details. Columns (1)-(3) report estimates for different sets of controls. T: time-varying controls; S-Y: state-year fixed effects. Standard errors in parentheses.

Figure A.21: *Robustness checks for employment in nursing*



Notes: Figure reports point estimates and 95 percent confidence intervals for equation (1.2), estimated using OLS. Robustness checks are described in Section 1.3.3. The dependent variable is an indicator for employment in nursing in 1920. Coefficients are multiplied by 100 and can be interpreted in percentage points. The sample is native-born white women, described in Section 1.3.1. All specifications include individual controls, town fixed effects and birth year-by-county fixed effects. Standard errors clustered at the town level.

Table A.11: *Count of nurses in the census by gender*

	(1)	(2)	(3)	(4)	(5)	(6)
	Professional nurses			All nurses		
	All	Women	Men	All	Women	Men
1870	132	56	76	9,352	8,730	622
1880	347	227	120	20,486	19,498	988
1900	9,650	8,898	752	79,330	74,037	5,293
1910	68,204	62,943	5,261	159,142	149,997	9,145
1920	106,059	102,054	4,005	237,180	228,740	8,440
1930	189,366	184,677	4,689	371,177	362,517	8,660
1940	356,188	333,227	22,961	441,232	413,525	27,707
1950	396,225	387,064	9,161	590,689	575,050	15,639
1960	565,300	550,200	15,100	808,980	785,100	23,880
1970	806,700	785,400	21,300	1,054,900	1,024,100	30,800
1980	1,247,340	1,195,400	51,940	1,659,260	1,593,200	66,060
1990	1,828,769	1,727,121	101,648	2,237,115	2,109,349	127,766
2000	2,195,959	2,027,042	168,917	2,756,707	2,548,766	207,941
2005	2,356,045	2,150,184	205,861	2,968,567	2,719,877	248,690
2010	2,705,442	2,455,822	249,620	3,320,370	3,024,677	295,693
2015	3,113,789	2,767,984	345,805	3,905,085	3,462,736	442,349

Notes: Table reports the number of nurses aged 15-64 by gender for the years 1870-2015. Columns 1-3 report professional nurses only. Columns 4-6 report all nurses (professional, practical, and student nurses). Counts from 1870 to 1940 use the 1950 classification of occupations. Counts from 1950 onward use the codes in Table A.7. Data come from the complete-count census for 1870-1940; 1% samples for 1950 and 1970; 5% samples for 1960, 1980, 1990, and 2000; American Community Survey for 2005, 2010, and 2015. Person weights are used for the 1950-2015 samples.

Table A.12: *Descriptive statistics, 1930*

	(1) Full sample	(2) Treated = 1	(3) Treated = 0
Childhood town, 1900			
Ln(population)	8.18	11.61	9.53
Region = North	0.26	0.40	0.49
Region = South	0.43	0.41	0.30
Region = Midwest	0.26	0.14	0.16
Region = West	0.06	0.05	0.05
Father characteristics, 1900			
Literate	0.93	0.96	0.95
Employed	0.92	0.87	0.90
Occupation score | Employed	21.21	27.78	24.21
Mother characteristics, 1900			
Number of children	4.23	3.97	4.14
Literate	0.93	0.95	0.94
Employed	0.04	0.04	0.04
Occupation score | Employed	15.48	17.96	16.82
Adult characteristics, 1930			
Married	0.80	0.71	0.74
Employed	0.21	0.27	0.25
Nurse | Employed	0.04	0.03	0.03
Occupation score | Employed	22.08	23.43	22.95
Unique places	33,210	1,903	4,577
Observations	5,232,365	777,766	548,654

Notes: Table reports descriptive statistics for the 1930 linked sample, described in Section 1.3.1. Statistics are weighted using inverse propensity scores to adjust for the likelihood of linkage. Column (1) reports means for the full sample. Column (2) reports means for treated women (e.g., women who were within five miles of an opening at ages 16-20). Column (3) reports means for the remainder of the final analysis sample (see Section 1.3.2). Father and mother characteristics are for women with an identifiable father or mother in the 1900 census. Nurse is an indicator for work in any nursing occupation. Occupation score and nurse variables are conditional on gainful employment.

Table A.13: *Placebo tests with father and mother characteristics, 1930*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Father characteristics			Mother characteristics			
	Literate	Occ score	Medicine	N children	Literate	Occ score	Medicine
Opening <5 mi. at ages 16-20	0.016	0.075	0.020	−0.013	−0.020	−0.044**	−0.004
	(0.118)	(0.061)	(0.037)	(0.011)	(0.123)	(0.018)	(0.006)
Controls	No	No	No	No	No	No	No
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	95.76	23.18	0.69	4.02	95.07	0.69	0.04
Observations	1,227,495	1,227,495	1,227,495	1,275,292	1,275,292	1,275,292	1,275,292

Notes: Table reports results for equation (1.2), estimated using OLS. Placebo outcomes are father and mother characteristics from the 1900 census: *Number of children* (as reported by the mother), *Literate* (father/mother can read and write), *Occ score* (father/mother occupation score), and *Medicine* (father/mother works in medicine). Coefficients on binary variables (*Literate* and *Medicine*) are multiplied by 100 and can be interpreted in percentage points. The sample is native-born white women, described in Section 1.3.1. Sample is restricted to women with an identifiable father (columns (1)-(3)) or mother (columns (4)-(7)). All specifications include town fixed effects and birth year-by-county fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table A.14: *Effects on employment and marriage, 1920*

	(1)	(2)	(3)	(4)
	Employed		Married	
Panel A: Full sample				
Opening <5 mi. at ages 16-20	0.196	0.219	0.175	0.168
	(0.213)	(0.213)	(0.221)	(0.222)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Mean outcome	39.78	39.78	50.86	50.86
Observations	1,646,657	1,646,657	1,646,657	1,646,657
Panel B: Married				
Opening <5 mi. at ages 16-20	0.127	0.129		
	(0.165)	(0.165)		
Controls	No	Yes		
Fixed effects	Yes	Yes		
Mean outcome	7.57	7.57		
Observations	837,445	837,445		
Panel C: Single				
Opening <5 mi. at ages 16-20	−0.202	−0.174		
	(0.340)	(0.338)		
Controls	No	Yes		
Fixed effects	Yes	Yes		
Mean outcome	73.11	73.11		
Observations	809,212	809,212		

Notes: Table reports results for equation (1.2), estimated using OLS. The dependent variables in columns (1) and (3) are indicators for gainful employment and marriage in 1920, respectively. Columns (2) and (4) report results with individual controls added. Coefficients are multiplied by 100 and can be interpreted in percentage points. The sample is native-born white women, as described in Section 1.3.1. Panel A reports results from the full sample. Panels B and C report results for women who are currently marriage or single, respectively. All specifications include town fixed effects and birth year-by-county fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table A.15: *Effects of proximity on entrance into healthcare, 1920*

	(1)	(2)	(3)	(4)
			Health	Health
	Attendant	Physician	professional	semi-pro.
Panel A: Full sample				
Opening <5 mi. at ages 16-20	0.006	−0.004	0.004	0.005
	(0.013)	(0.008)	(0.011)	(0.007)
Controls	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes
Mean outcome	0.08	0.03	0.05	0.02
Observations	1,646,657	1,646,657	1,646,657	1,646,657
Panel B: Employed				
Opening <5 mi. at ages 16-20	0.016	−0.006	0.015	0.009
	(0.043)	(0.026)	(0.035)	(0.023)
Controls	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes
Mean outcome	0.21	0.06	0.14	0.06
Observations	655,027	655,027	655,027	655,027

Notes: Table reports results for equation (1.2), estimated using OLS. The dependent variables are indicators for attendant, physician, health professional (physician, dentist, optometrist, osteopath, or pharmacist), and health semi-professional (chiropractor, dietitian, medical technician, therapist, or midwife) in 1920. Coefficients are multiplied by 100 and can be interpreted in percentage points. The sample is native-born white women, as described in Section 1.3.1. Panel A reports results for the full sample. Panel B reports results for women who are gainfully employed. All specifications include individual controls, town fixed effects, and birth year-by-county fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table A.16: *Effect of proximity on entrance into healthcare by father's occupation, 1920*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Nurse subcategories		Other health occupations			
	Nurse	Trained	Practical	Attendant	Physician	Professional	Semi-pro.
Panel A: Father - professional							
Opening <5 mi. at ages 16-20	0.533**	0.513***	0.020	−0.017	0.005	0.096	−0.022
	(0.212)	(0.179)	(0.118)	(0.047)	(0.059)	(0.072)	(0.034)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	0.93	0.65	0.28	0.08	0.07	0.13	0.04
Observations	194,546	194,546	194,546	194,546	194,546	194,546	194,546
Panel B: Father - routine/manual							
Opening <5 mi. at ages 16-20	0.161**	0.102*	0.059	−0.012	−0.000	0.005	0.008
	(0.072)	(0.055)	(0.047)	(0.018)	(0.010)	(0.014)	(0.009)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	0.79	0.51	0.28	0.08	0.02	0.04	0.02
Observations	959,326	959,326	959,326	959,326	959,326	959,326	959,326
Panel C: Father - farmer							
Opening <5 mi. at ages 16-20	−0.008	−0.021	0.013	0.033	0.001	−0.007	−0.010
	(0.099)	(0.083)	(0.060)	(0.032)	(0.013)	(0.017)	(0.013)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	0.72	0.47	0.24	0.07	0.01	0.02	0.01
Observations	194,665	194,665	194,665	194,665	194,665	194,665	194,665

Notes: Table reports results for equation (1.2), estimated using OLS. The dependent variables are indicators for nurse, trained nurse, practical nurse, attendant, physician, health professional (physician, dentist, optometrist, osteopath, or pharmacist), and health semi-professional (chiropractor, dietitian, medical technician, therapist, or midwife) in 1920. Coefficients are multiplied by 100 and can be interpreted in percentage points. The sample is native-born white women, as described in Section 1.3.1. Panels A, B, and C report results for women with fathers in professional, routine/manual, and farming occupations, respectively. All specifications include individual controls, town fixed effects and birth year-by-county fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table A.17: *Effect of proximity on entrance into healthcare - single women, 1920*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Nurse subcategories		Other health occupations			
	Nurse	Trained	Practical	Attendant	Physician	Professional	Semi-pro.
Panel A: Single							
Opening <5 mi. at ages 16-20	0.338***	0.257***	0.081	0.032	−0.001	0.014	−0.004
	(0.115)	(0.089)	(0.069)	(0.030)	(0.018)	(0.024)	(0.016)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	1.59	1.06	0.53	0.15	0.04	0.09	0.04
Observations	809,212	809,212	809,212	809,212	809,212	809,212	809,212
Panel B: Single and employed							
Opening <5 mi. at ages 16-20	0.532***	0.412***	0.119	0.041	0.002	0.020	−0.004
	(0.171)	(0.132)	(0.104)	(0.046)	(0.028)	(0.037)	(0.024)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	2.17	1.45	0.72	0.21	0.06	0.12	0.06
Observations	591,625	591,625	591,625	591,625	591,625	591,625	591,625

Notes: Table reports results for equation (1.2), estimated using OLS. The dependent variables are indicators for nurse, trained nurse, practical nurse, attendant, physician, health professional (physician, dentist, optometrist, osteopath, or pharmacist), and health semi-professional (chiropractor, dietitian, medical technician, therapist, or midwife) in 1920. Coefficients are multiplied by 100 and can be interpreted in percentage points. The sample is native-born white women, as described in Section 1.3.1. Panel A reports results for single women. Panel B reports results for single women who are gainfully employed. All specifications include individual controls, town fixed effects and birth year-by-county fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table A.18: *Effect of proximity on entrance into healthcare - married women, 1920*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Nurse subcategories		Other health occupations			
	Nurse	Trained	Practical	Attendant	Physician	Professional	Semi-pro.
Panel A: Married							
Opening <5 mi. at ages 16-20	0.014	0.002	0.012	−0.015**	−0.004	0.001	0.004
	(0.021)	(0.013)	(0.017)	(0.007)	(0.006)	(0.008)	(0.005)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	0.09	0.04	0.05	0.01	0.01	0.02	0.01
Observations	837,445	837,445	837,445	837,445	837,445	837,445	837,445
Panel B: Married and employed							
Opening <5 mi. at ages 16-20	0.392	0.041	0.351	−0.301**	−0.053	−0.081	0.014
	(0.348)	(0.218)	(0.272)	(0.119)	(0.077)	(0.106)	(0.079)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	1.19	0.58	0.61	0.19	0.15	0.28	0.09
Observations	63,402	63,402	63,402	63,402	63,402	63,402	63,402

Notes: Table reports results for equation (1.2), estimated using OLS. The dependent variables are indicators for nurse, trained nurse, practical nurse, attendant, physician, health professional (physician, dentist, optometrist, osteopath, or pharmacist), and health semi-professional (chiropractor, dietitian, medical technician, therapist, or midwife) in 1920. Coefficients are multiplied by 100 and can be interpreted in percentage points. The sample is native-born white women, as described in Section 1.3.1. Panel A reports results for married women. Panel B reports results for married women who are gainfully employed. All specifications include individual controls, town fixed effects and birth year-by-county fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table A.19: *Effects of proximity on entrance into common occupations, 1920*

	(1) Teacher	(2) Clerk	(3) Bookkeeper	(4) Secretary	(5) Laborer
Panel A: Full sample					
Opening <5 mi. at ages 16-20	−0.048	−0.039	−0.026	0.149	−0.053
	(0.099)	(0.090)	(0.073)	(0.107)	(0.045)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes
Mean outcome	3.70	4.80	3.18	7.66	0.76
Observations	1,646,657	1,646,657	1,646,657	1,646,657	1,646,657
Panel B: Employed					
Opening <5 mi. at ages 16-20	−0.122	−0.199	−0.336	−0.022	−0.016
	(0.299)	(0.253)	(0.232)	(0.303)	(0.140)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes
Mean outcome	9.31	12.06	7.99	19.25	1.91
Observations	655,027	655,027	655,027	655,027	655,027

Notes: Table reports results for equation (1.2), estimated using OLS. The dependent variables are indicators for working in other common occupations in 1920: teacher, clerk, bookkeeper, secretary, and laborer (including farm laborer). Coefficients are multiplied by 100 and can be interpreted in percentage points. The sample is native-born white women, as described in Section 1.3.1. Panel A reports results for the full sample. Panel B reports results for women who are gainfully employed. All specifications include individual controls, town fixed effects, and birth year-by-county fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table A.20: *Effects of proximity on occupational income by father's occupation, 1920*

	(1) Ln(occ. score)	(2) Ln(LIDO score)	(3) Song score
Panel A: Father - professional			
Opening <5 mi. at ages 16-20	0.008	0.008	0.188
	(0.010)	(0.011)	(0.646)
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Mean outcome	3.18	2.81	82.28
Observations	73,613	64,214	73,522
Panel B: Father - routine/manual			
Opening <5 mi. at ages 16-20	0.006	0.004	0.639**
	(0.004)	(0.005)	(0.296)
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Mean outcome	3.14	2.78	73.76
Observations	419,935	363,885	419,560
Panel C: Father - farmer			
Opening <5 mi. at ages 16-20	0.001	0.013	0.664
	(0.012)	(0.013)	(0.717)
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Mean outcome	2.99	2.60	69.74
Observations	41,641	36,327	41,222

Notes: Table reports results for equation (1.2), estimated using OLS. The dependent variables are the natural log of occupation score, the natural log of LIDO score, and Song score (see Appendix Section A.3.2). The sample is native-born white women, as described in Section 1.3.1. Panels A-C report results for employed women whose fathers work in professional, routine/manual, and farming occupations, respectively. All specifications include individual controls, town fixed effects, and birth year-by-county fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table A.21: *Effects on employment and marriage, 1930*

	(1)	(2)	(3)	(4)
	Employed		Married	
Panel A: Full sample				
Opening <5 mi. at ages 16-20	−0.354*	−0.345	0.647***	0.642***
	(0.211)	(0.212)	(0.210)	(0.211)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Mean outcome	26.09	26.09	72.30	72.30
Observations	1,326,420	1,326,420	1,326,420	1,326,420
Panel B: Married				
Opening <5 mi. at ages 16-20	0.031	0.033		
	(0.164)	(0.163)		
Controls	No	Yes		
Fixed effects	Yes	Yes		
Mean outcome	9.34	9.34		
Observations	959,031	959,031		
Panel C: Single				
Opening <5 mi. at ages 16-20	−0.019	−0.020		
	(0.511)	(0.511)		
Controls	No	Yes		
Fixed effects	Yes	Yes		
Mean outcome	69.82	69.82		
Observations	367,389	367,389		

Notes: Table reports results for equation (1.2), estimated using OLS. The dependent variables in columns (1) and (3) are indicators for gainful employment and marriage in 1930, respectively. Columns (2) and (4) report results with individual controls added. Coefficients are multiplied by 100 and can be interpreted in percentage points. The sample is native-born white women, as described in Section 1.3.1. Panel A reports results from the full sample. Panels B and C report results for women who are currently marriage or single, respectively. All specifications include town fixed effects and birth year-by-county fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table A.22: *Effects of proximity on entrance into healthcare, 1930*

	(1)	(2)	(3)	(4)
			Health	Health
	Attendant	Physician	professional	semi-pro.
Panel A: Full sample				
Opening <5 mi. at ages 16-20	0.007	−0.022**	−0.009	0.010
	(0.012)	(0.011)	(0.014)	(0.012)
Controls	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes
Mean outcome	0.07	0.04	0.07	0.06
Observations	1,326,420	1,326,420	1,326,420	1,326,420
Panel B: Employed				
Opening <5 mi. at ages 16-20	0.018	−0.100**	−0.046	0.053
	(0.057)	(0.051)	(0.063)	(0.052)
Controls	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes
Mean outcome	0.29	0.17	0.27	0.21
Observations	346,121	346,121	346,121	346,121

Notes: Table reports results for equation (1.2), estimated using OLS. The dependent variables are indicators for attendant, physician, health professional (physician, dentist, optometrist, osteopath, or pharmacist), and health semi-professional (chiropractor, dietitian, medical technician, therapist, or midwife) in 1930. Coefficients are multiplied by 100 and can be interpreted in percentage points. The sample is native-born white women, as described in Section 1.3.1. Panel A reports results for the full sample. Panel B reports results for women who are gainfully employed. All specifications include individual controls, town fixed effects, and birth year-by-county fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table A.23: *Effect of proximity on entrance into healthcare - married women, 1930*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Nurse subcategories		Other health occupations			
	Nurse	Trained	Practical	Attendant	Physician	Professional	Semi-pro.
Panel A: Married							
Opening <5 mi. at ages 16-20	0.052*	0.008	0.043**	0.000	−0.020*	−0.006	0.004
	(0.029)	(0.022)	(0.020)	(0.008)	(0.011)	(0.013)	(0.009)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	0.19	0.11	0.09	0.03	0.02	0.04	0.02
Observations	959,031	959,031	959,031	959,031	959,031	959,031	959,031
Panel B: Married and employed							
Opening <5 mi. at ages 16-20	0.640*	0.136	0.504**	−0.023	−0.331**	−0.203	0.052
	(0.346)	(0.255)	(0.240)	(0.088)	(0.134)	(0.159)	(0.094)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	2.09	1.18	0.91	0.28	0.24	0.42	0.19
Observations	89,616	89,616	89,616	89,616	89,616	89,616	89,616

Notes: Table reports results for equation (1.2), estimated using OLS. The dependent variables are indicators for nurse, trained nurse, practical nurse, attendant, physician, health professional (physician, dentist, optometrist, osteopath, or pharmacist), and health semi-professional (chiropractor, dietitian, medical technician, therapist, or midwife) in 1930. Coefficients are multiplied by 100 and can be interpreted in percentage points. The sample is native-born white women, as described in Section 1.3.1. Panel A reports results for married women. Panel B reports results for married women who are gainfully employed. All specifications include individual controls, town fixed effects and birth year-by-county fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table A.24: *Effects of proximity on husband's occupation*

	(1) Husband ln(occ. score)	(2) Husband in health occ.
Panel A: Married women		
Opening <5 mi. at ages 16-20	0.002	0.093
	(0.005)	(0.071)
Controls	Yes	Yes
Fixed effects	Yes	Yes
Mean outcome	3.18	1.69
Observations	920,933	920,933
Panel B: Married women, husband employed		
Opening <5 mi. at ages 16-20	−0.003	0.095
	(0.002)	(0.074)
Controls	Yes	Yes
Fixed effects	Yes	Yes
Mean outcome	3.37	1.80
Observations	867,878	867,878

Notes: Table reports results for equation (1.2), estimated using OLS. The dependent variables are husband log occupational income score (column (1)) and an indicator for whether husband works in a professional health occupation (column (2)). Coefficient in column (2) is multiplied by 100 and can be interpreted in percentage points. The sample is native-born married white women, as described in Section 1.3.1. All specifications include individual controls, town, state-by-cohort, and urban group-by-cohort fixed effects. Inverse propensity score weights are applied to adjust for the likelihood of linkage. Standard errors clustered at the town level in parentheses. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Appendix B

Appendix to Chapter 2

B.1 State Vaccine Legislation

California

In June 2015, the California legislature passed Senate Bill 277, which repealed the personal beliefs exemption (PBE) for school-entry vaccines. The law took effect on January 1st, 2016. See the California law here. The law applies to children receiving “class-based instruction", which includes public schools and private schools. The law does not apply to children who are homeschooled.

- For students entering kindergarten, the law requires up-to-date vaccination for DTaP, MMR, Polio, Hepatitis B, and Varicella.
- Children with a PBE filed before January 2016 can keep the exemption until they reach the next vaccine checkpoint (kindergarten or 7th grade).
- Children with an Individualized Education Plan (IEP) cannot be denied special education services and may be eligible for a PBE.
- The law broadened the scope of licensed medical providers to issue a medical exemption. Effective January 1st, 2021, all medical exemptions must be issued through the California Immunization Registry - Medical Exemption (CAIR-ME) website. See the legislation here.

Connecticut

Connecticut passed Public Act No. 21-6 on April 28th, 2021. Public Act No. 21-6 repealed non-medical vaccine exemptions for children in pre K-12 schools, child care centers, group child care homes, family child care homes, and higher education institutions. See the Connecticut law here. The law took effect immediately and applies to children attending public or private schools (not children who are homeschooled).

- For students entering kindergarten, the law requires up-to-date vaccination for DTaP, MMR, Polio, Hepatitis A, Hepatitis B, and Varicella.
- Children in grades K-12 with a non-medical exemption in place prior to April 28th, 2021 can keep their exemption. However, children in pre-school with non-medical exemptions must comply with the new law. Children may be given additional time to become compliant, as

long as there is written declaration from the child's physician that an alternative vaccine schedule is recommended.

- The law makes no exception for children with an Individualized Education Plan (IEP).

Maine

In February 2019, the Maine legislature introduced LD 798, a bill that proposed to repeal non-medical vaccine exemptions. The law was signed in May 2019 and took effect on September 1st, 2021. See the Maine law here. The law applies to children attending public or private schools (not children who are homeschooled).

- For students entering kindergarten, the law requires up-to-date vaccination for DTaP, MMR, Polio, and Varicella (Hepatitis B is not required).
- Children with an Individualized Education Plan (IEP) were allowed to obtain or keep a non-medical vaccine exemption, as long as the IEP was in place as of September 1st, 2021. There was no such allowance for non-IEP children.
- Medical exemptions may only be provided by a licensed physician, nurse practitioner, or physician assistant. Children may also show proof of immunity resulting from prior infection.

New York

In January 2019, the New York legislature introduced S2994A, a bill that proposed to repeal non-medical vaccine exemptions for children attending day care and public, private, or religious schools (pre-K through high school). The bill was signed into law on June 13th, 2019 and applied to the 2019 school year. See the New York law here. The law applies to children attending public, private, or parochial schools (not children who are homeschooled).

- For students entering kindergarten, the law requires up-to-date vaccination for DTaP, MMR, Polio, Hepatitis B, and Varicella.
- In practice, students are required to have the first age-appropriate dose of the required vaccines within 14 days of the first day of school or day care to remain in school. Within 30

days, parents are required to show documentation of vaccine follow-up appointments (see here).

- Children with existing non-medical exemptions are not allowed to retain them and must comply with the new law. The law makes no exception for children with an Individualized Education Plan (IEP).
- Additional vaccines are required for day care and pre-K in the state of New York, including Haemophilus influenzae type B (HiB) and Pneumococcal Conjugate Vaccine (PCV).

Washington

In May 2019, the governor of Washington signed a law that repealed personal or philosophical exemptions from the MMR vaccine. See the Washington law here. The law did not affect other vaccines, and medical and religious exemptions can still be granted for MMR. The law took effect on July 28th, 2019 and applies to children attending public or private schools (not children who are homeschooled).

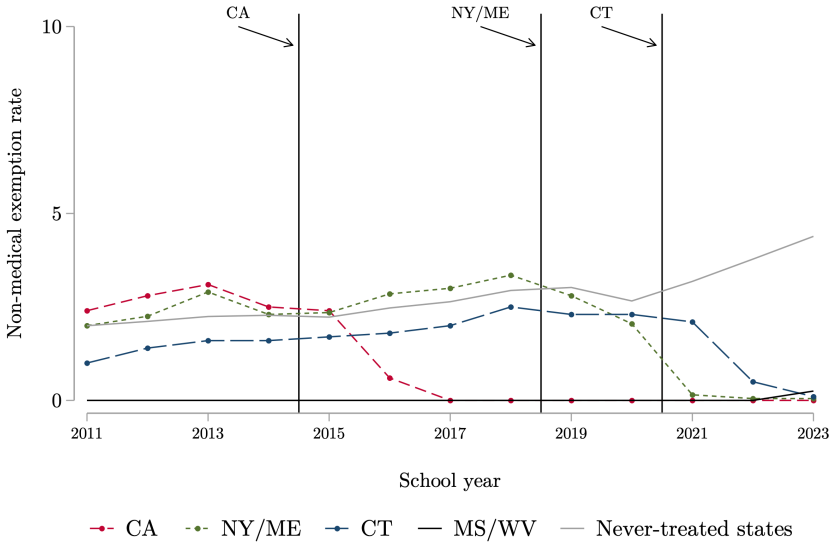
Vermont

In May 2015, the governor of Vermont signed a law that repealed philosophical vaccine exemptions for children in school. See the Vermont law here. The law did not affect religious exemptions and took effect on July 1st, 2016 for children attending public or private schools (not children who are homeschooled).

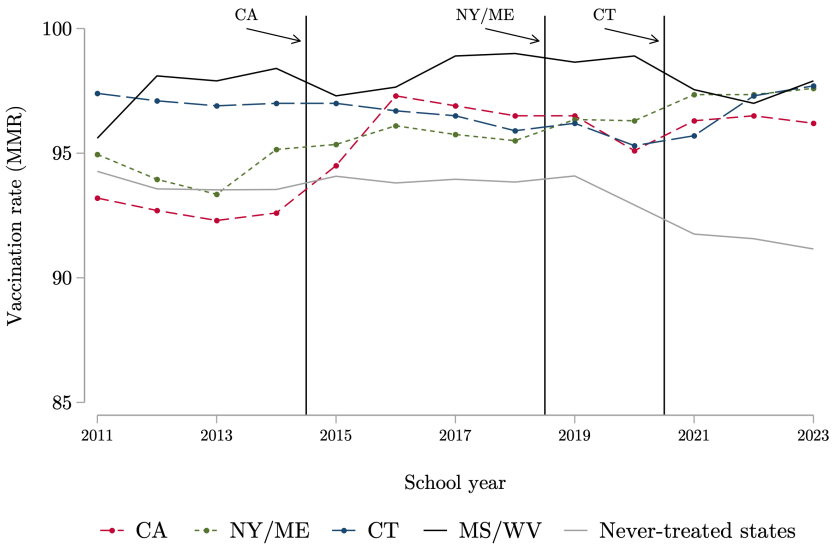
B.2 Supplementary Tables and Figures

Figure B.1: *Trends by treatment year*

(a) *Non-medical exemptions*

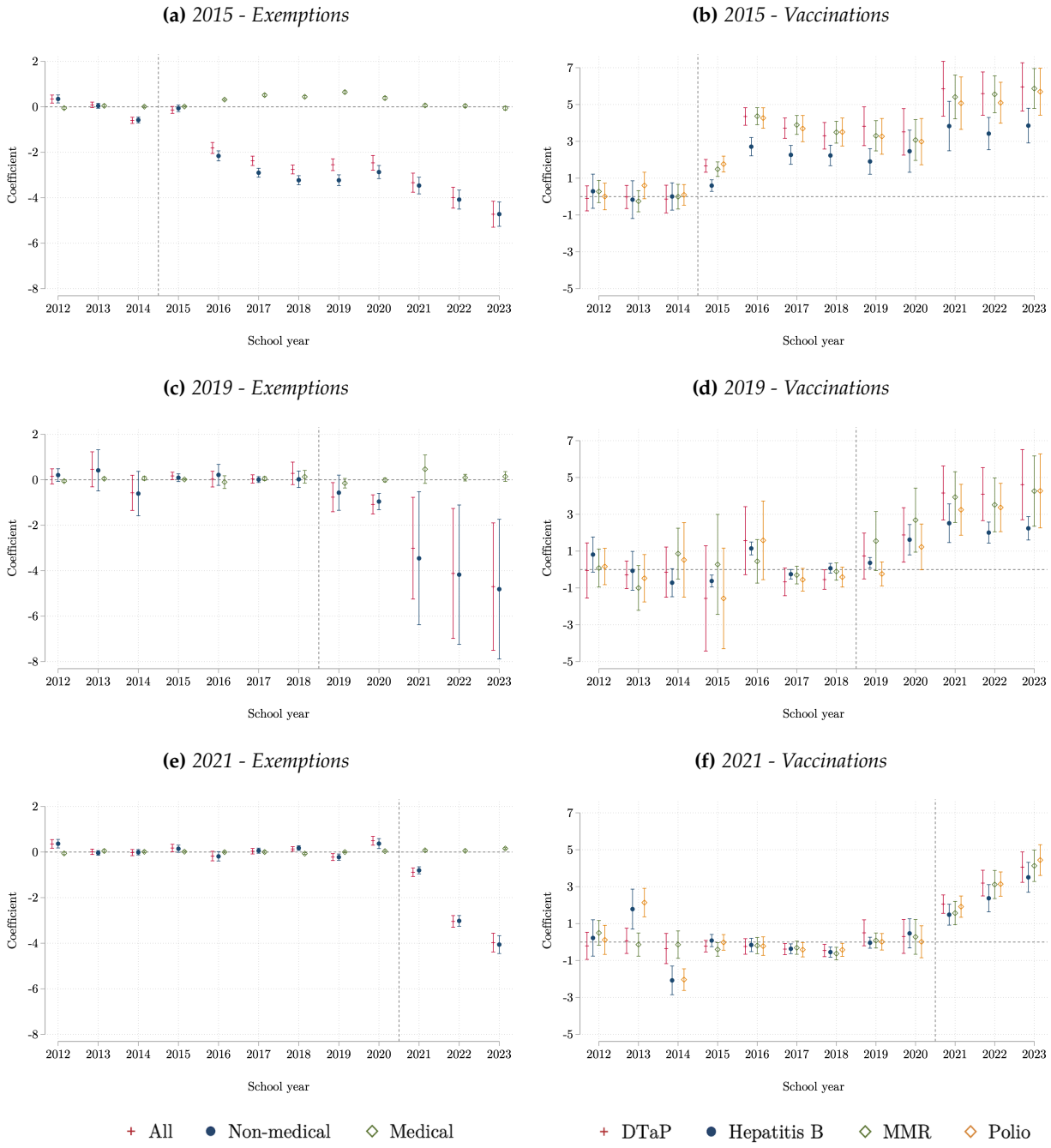


(b) *Vaccinations (MMR)*



Notes: Figure shows trends in non-medical exemption rates and vaccination (MMR) rates in panels (a) and (b). Data are measured for children entering kindergarten from 2011 to 2023. Trend lines are plotted separately for states that repealed non-medical exemptions in different years. Vertical lines indicate the year that the law was passed. The groups of states that never allowed non-medical exemptions (Mississippi and West Virginia) and all other states are plotted in black and gray. Data are sourced from the CDC (see Section 2.3.2 for details).

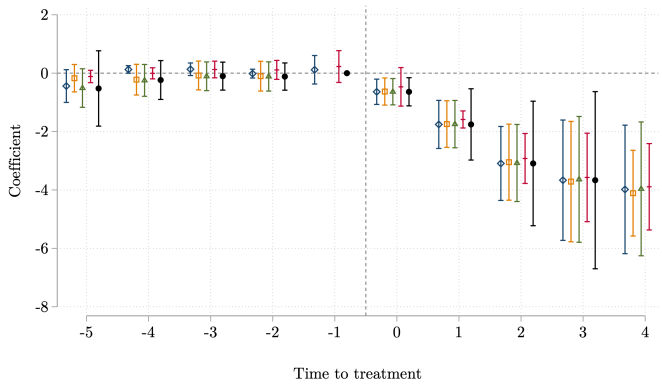
Figure B.2: *Exemption repeal: Estimates by treatment year*



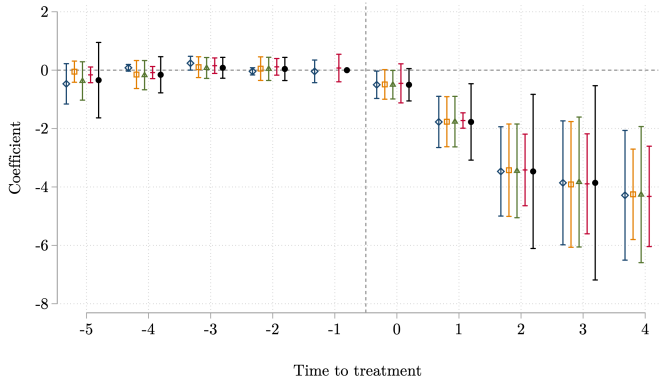
Notes: Figure shows event study coefficients from equation (2.1) for states treated in 2015, 2019, and 2021. Coefficients are estimated using the event study methodology of Callaway and Sant'Anna (2021). 95 percent confidence intervals are shown with standard errors clustered at the state level. The x-axis is displayed in school years. Results are shown for states treated in 2015 in Panels (a) and (b), 2019 in Panels (c) and (d), and 2021 in Panels (e) and (f). The dependent variables are exemption and vaccination rates for children entering kindergarten (see legend). Data are sourced from the CDC (see Section 2.3.2 for details).

Figure B.3: *Exemption repeal and state exemption rates: Robustness to different estimators*

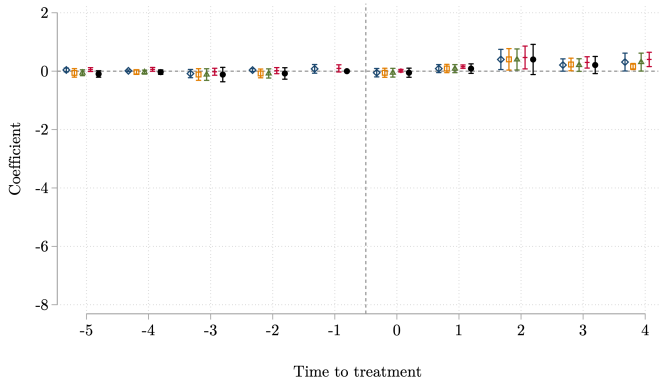
(a) *All exemptions*

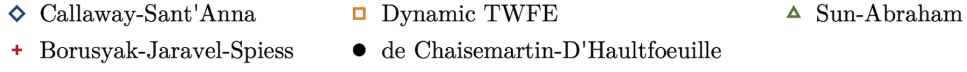


(b) *Non-medical exemptions*



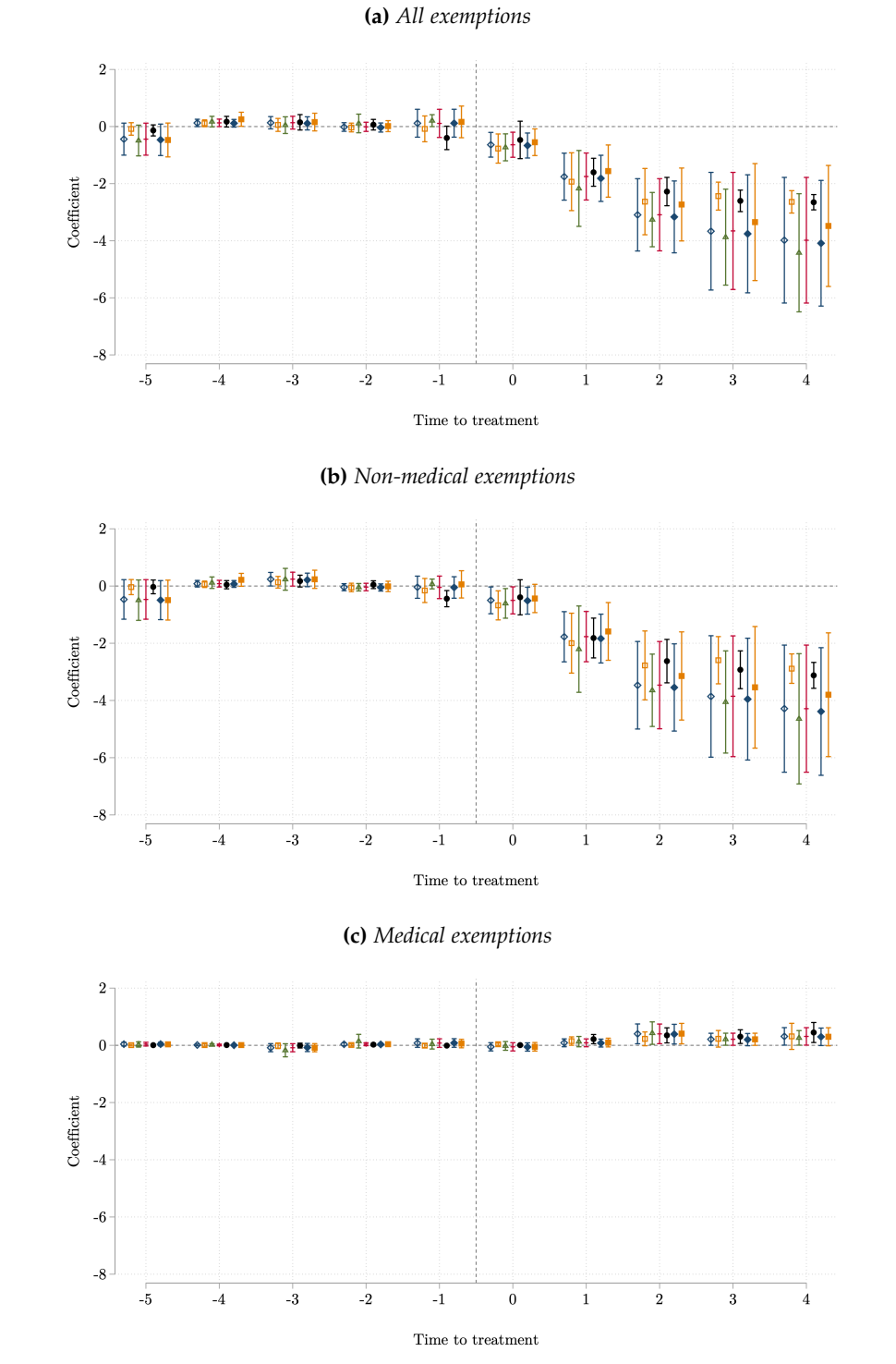
(c) *Medical exemptions*





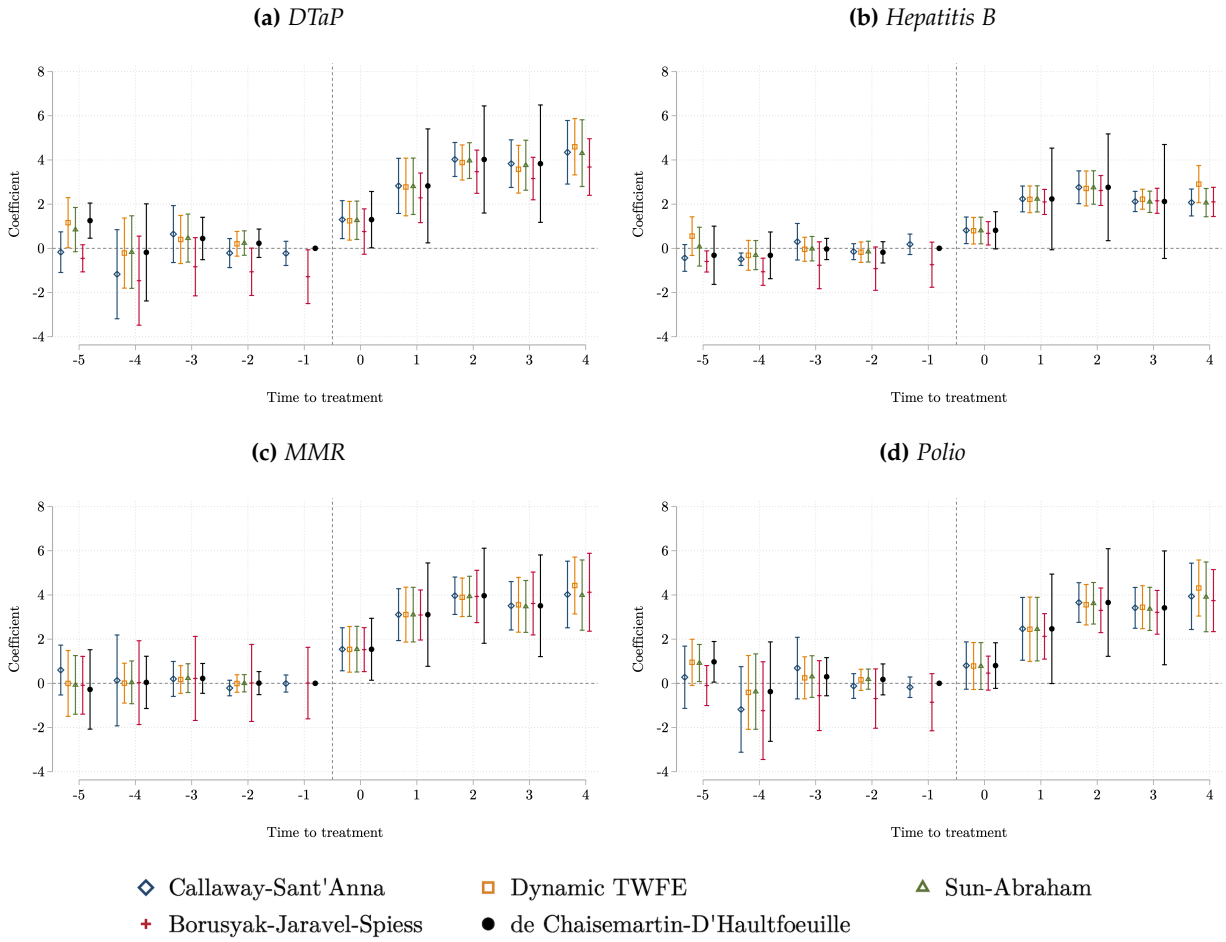
Notes: Figure shows robustness of event study results to different estimators. The outcomes in Panels (a), (b), and (c) are the overall exemption rate, the non-medical exemption rate, and the medical exemption rate. Event study estimators are sourced from Borusyak, Jaravel, and Spiess (2024), de Chaisemartin and D'Haultfœuille (2024), Callaway and Sant'Anna (2021), and Sun and Abraham (2021). The dynamic two-way fixed effects approach is estimated using the *xtevent* package (Freyaldenhoven et al., 2021).

Figure B.4: *Exemption repeal and state exemption rates: Additional robustness checks*



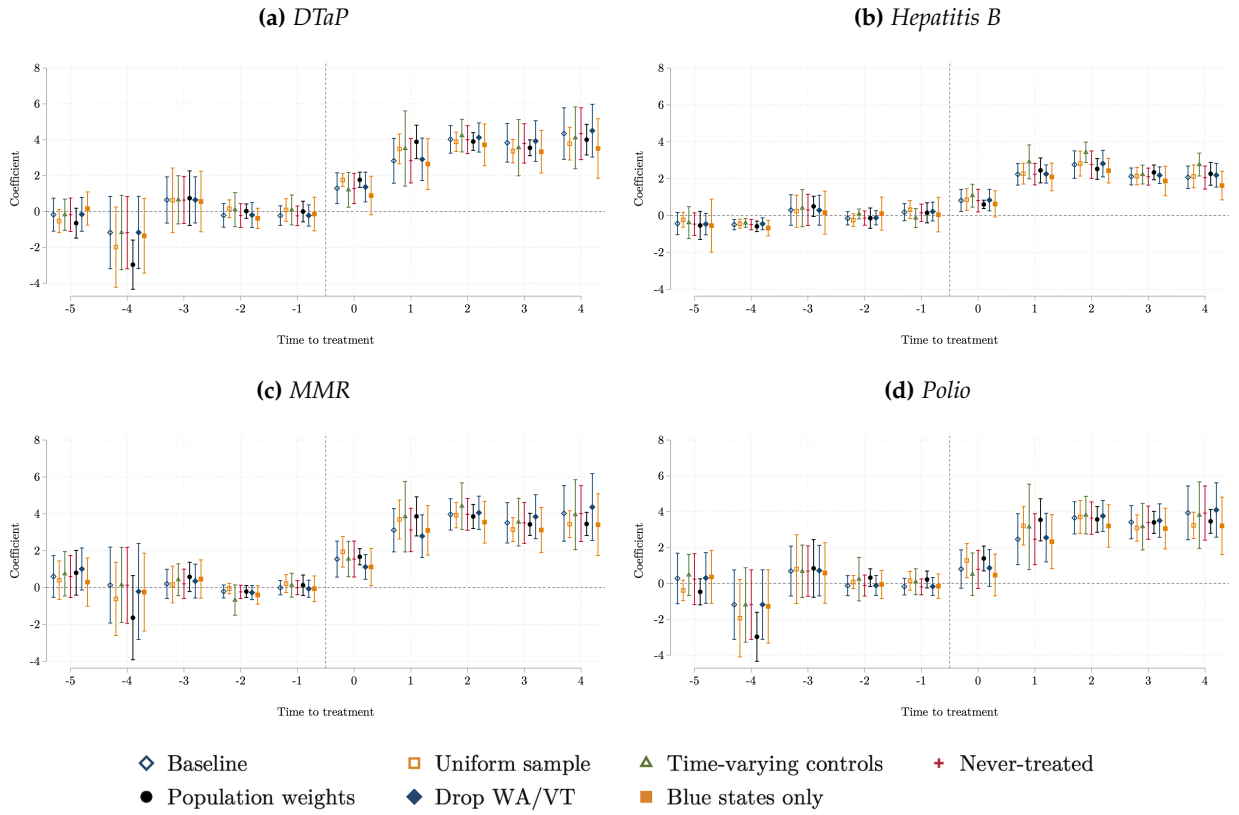
Notes: Figure reports additional robustness checks for state exemption rates. The outcomes in Panels (a), (b), and (c) are the overall exemption rate, the non-medical exemption rate, and the medical exemption rate. See Section 2.4.2 for a description of each robustness check.

Figure B.5: *Exemption repeal and state vaccination rates: Robustness to different estimators*



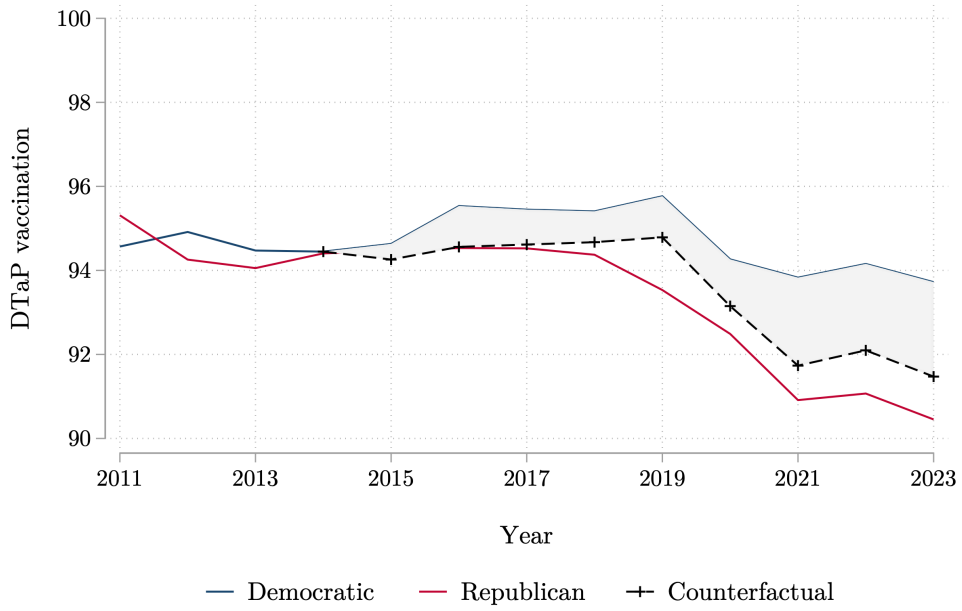
Notes: Figure shows robustness of event study results to different estimators. The outcomes in Panels (a), (b), (c), and (d) are the vaccination rates for DTaP, Hepatitis B, MMR, and Polio. Event study estimators are sourced from Borusyak, Jaravel, and Spiess (2024), de Chaisemartin and D'Haultfœuille (2024), Callaway and Sant'Anna (2021), and Sun and Abraham (2021). The dynamic two-way fixed effects approach is estimated using the *xtevent* package (Freyaldenhoven et al., 2021).

Figure B.6: *Exemption repeal and state vaccination rates: Additional robustness checks*



Notes: Figure reports additional robustness checks for state vaccination rates. The outcomes in Panels (a), (b), (c), and (d) are the vaccination rates for DTaP, Hepatitis B, MMR, and Polio. See Section 2.4.2 for a description of each robustness check.

Figure B.7: *Observed and counterfactual vaccination gap between Democratic and Republican states*



Notes: Figure shows the population-weighted trends in DTaP vaccination rates among Democratic states in blue and Republican states in red (defined by the prevailing party in the 2016 presidential election). The black line and shaded region show the counterfactual gap estimated using the estimated coefficients from equation (2.1). In particular, we subtract the group-specific coefficients shown in Figure B.2 from the observed DTaP vaccination rate to estimate the vaccination rate had exemption repeal not been adopted.

Table B.1: *Event study coefficients: Exemption and vaccination rates*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Exemption rate			Vaccination rate			
	All	Non-med	Med	DTaP	Hep B	MMR	Polio
T = -2	−0.02	−0.04	0.04	−0.22	−0.15	−0.21	−0.12
	(0.08)	(0.06)	(0.03)	(0.34)	(0.18)	(0.18)	(0.29)
T = -1	0.12	−0.04	0.08	−0.23	0.18	−0.01	−0.18
	(0.25)	(0.20)	(0.08)	(0.28)	(0.24)	(0.20)	(0.24)
T = 0	−0.64***	−0.50**	−0.05	1.30***	0.81***	1.54***	0.80
	(0.22)	(0.24)	(0.07)	(0.44)	(0.31)	(0.50)	(0.55)
T = +1	−1.76***	−1.77***	0.09	2.83***	2.23***	3.11***	2.47***
	(0.42)	(0.45)	(0.07)	(0.64)	(0.30)	(0.60)	(0.73)
T = +2	−3.09***	−3.47***	0.40**	4.02***	2.76***	3.96***	3.66***
	(0.65)	(0.78)	(0.18)	(0.39)	(0.38)	(0.43)	(0.46)
T = +3	−3.67***	−3.86***	0.21*	3.83***	2.12***	3.51***	3.42***
	(1.05)	(1.08)	(0.11)	(0.55)	(0.23)	(0.56)	(0.47)
T = +4	−3.98***	−4.29***	0.31**	4.35***	2.07***	4.02***	3.94***
	(1.12)	(1.13)	(0.16)	(0.73)	(0.31)	(0.77)	(0.77)
Mean outcome (T = -1)	3.20	2.88	0.28	94.72	96.30	93.94	94.88
State-year obs.	546	546	533	481	468	494	494

Notes: Table reports results from equation (2.1), estimated using the event study methodology of Callaway and Sant'Anna (2021). The outcomes are state exemption rates in columns (1)-(3) and vaccination rates in columns (4)-(7), derived from CDC data covering the 2011-2023 period (see Section 2.3.2). Sample is restricted in each column to a balanced panel of states. Coefficients are shown in percents and in event time for $T \in \{-2, 4\}$. Standard errors are clustered at the state level. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table B.2: *Regression discontinuity estimates of audit on non-compliance and vaccination rates*

	(1)	(2)	(3)	(4)	(5)	(6)
	Non-comp	Audited	DTaP	Hep B	MMR	Polio
Non-compliance > 10%	0.11	0.77	−1.34	−0.98	−0.91	−1.37
	(0.94)	(6.44)	(1.29)	(1.11)	(1.24)	(1.25)
Observations	5,543	5,543	5,543	5,543	5,543	5,543
Bandwidth	4.26	5.25	4.38	4.44	4.45	4.30
Effective obs.	880	1,804	880	880	880	880

Notes: Table reports regression discontinuity estimates for the effect of audit in 2021 on non-compliance and vaccination rates in 2022. Coefficients estimate the change in the outcome at the audit threshold of 10 percent non-compliance. Each column is a separate RD regression that uses the *rdrobust* package from Calonico et al. (2017). Outcomes are all shown in percents: Non-compliance, audit likelihood, and vaccination for DTaP, Hepatitis B, MMR, and Polio. See Section 2.5. Robust standard errors in parentheses. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

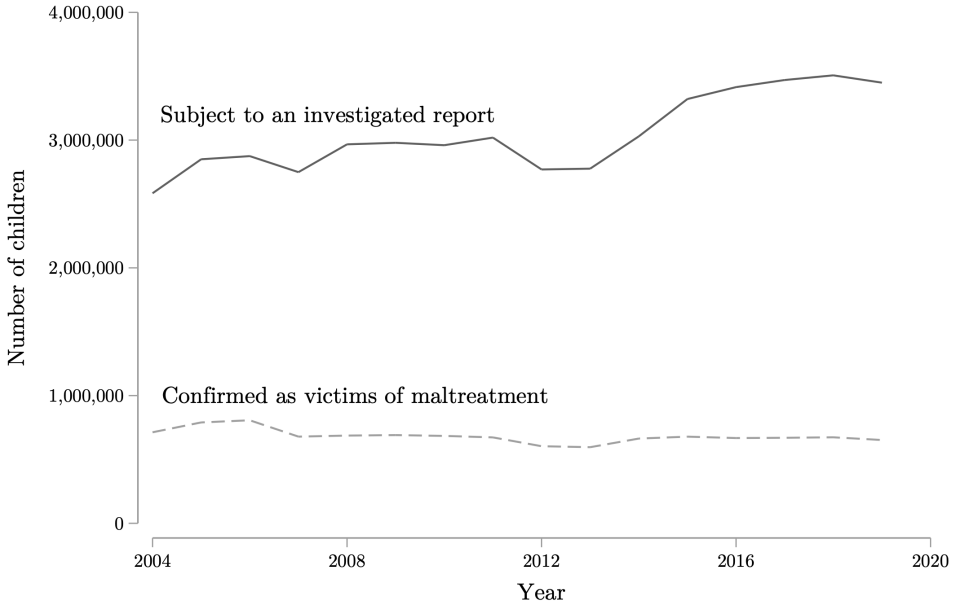
Appendix C

Appendix to Chapter 3

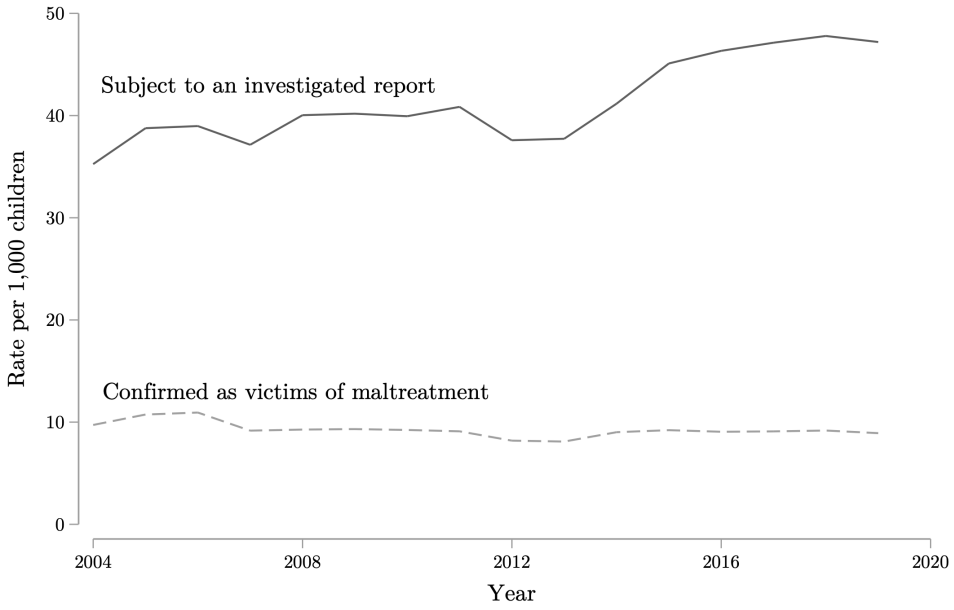
C.1 Supplementary Tables and Figures

Figure C.1: *Children subject to an investigated report and confirmed as victims of maltreatment*

(a) *Number of children over time*



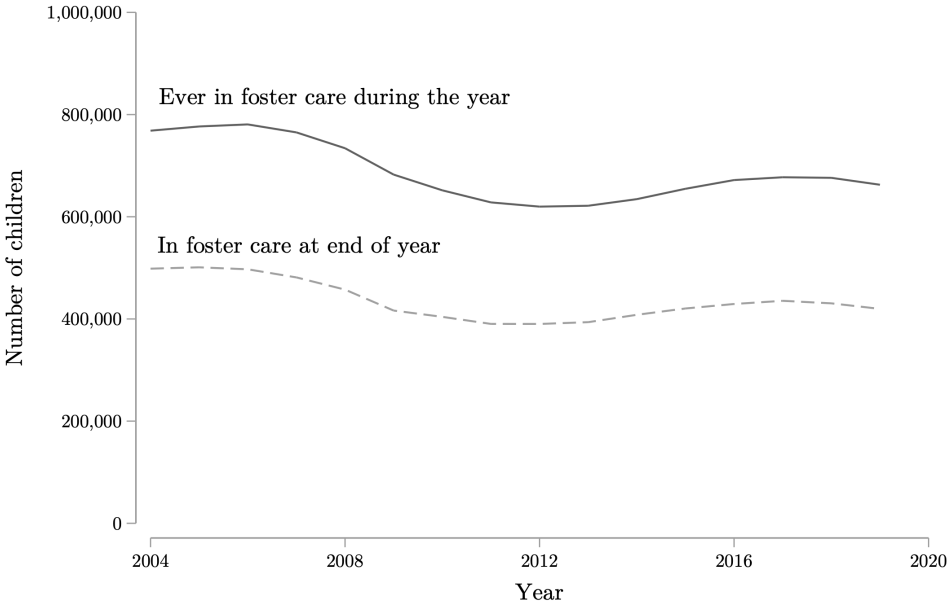
(b) *Rate per 1,000 children over time*



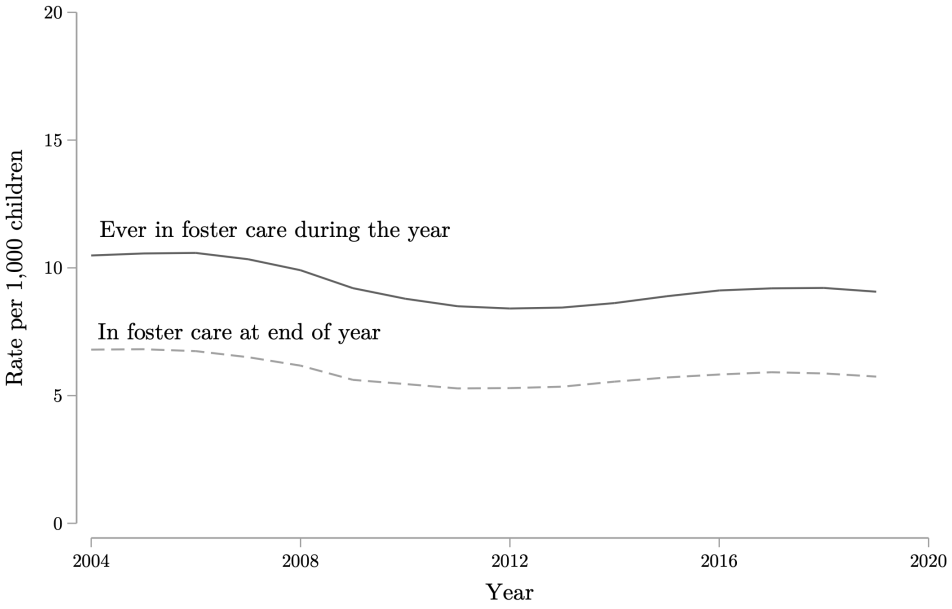
Notes: These statistics refer to unique children ages 0 to 17. Children are counted as confirmed victims of maltreatment if they are subject to a substantiated maltreatment report.

Figure C.2: *Children in foster care*

(a) *Number of children over time*

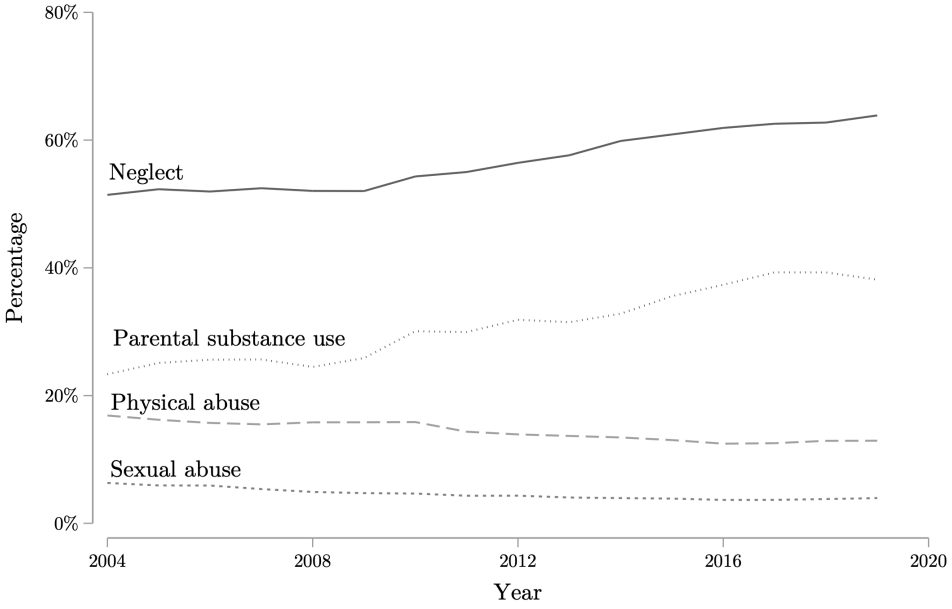


(b) *Rate per 1,000 children over time*



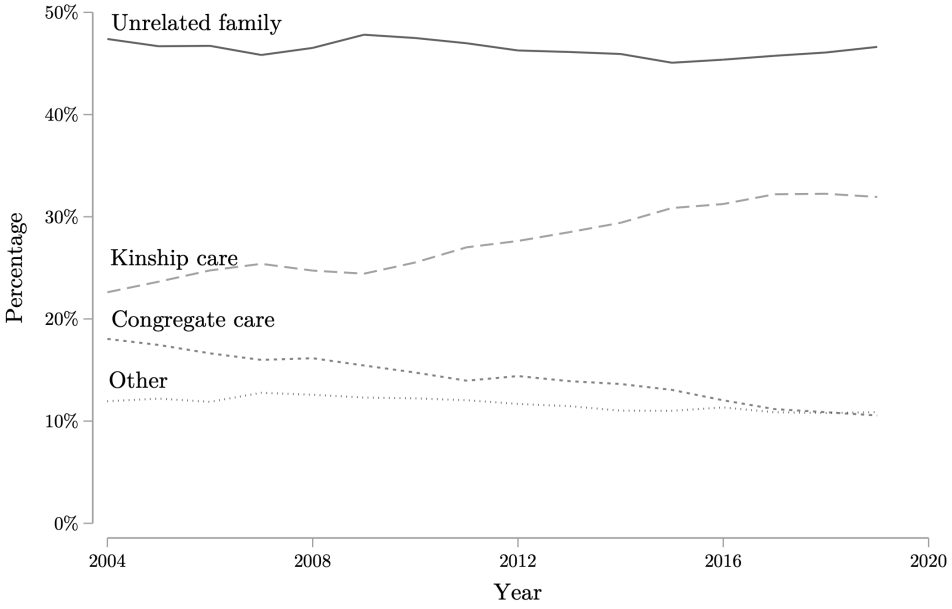
Notes: We restrict each series to children ages 0 to 17 who were ever in foster care during the fiscal year and who were in foster care at the end of the fiscal year.

Figure C.3: *Children entering foster care by reason for removal*



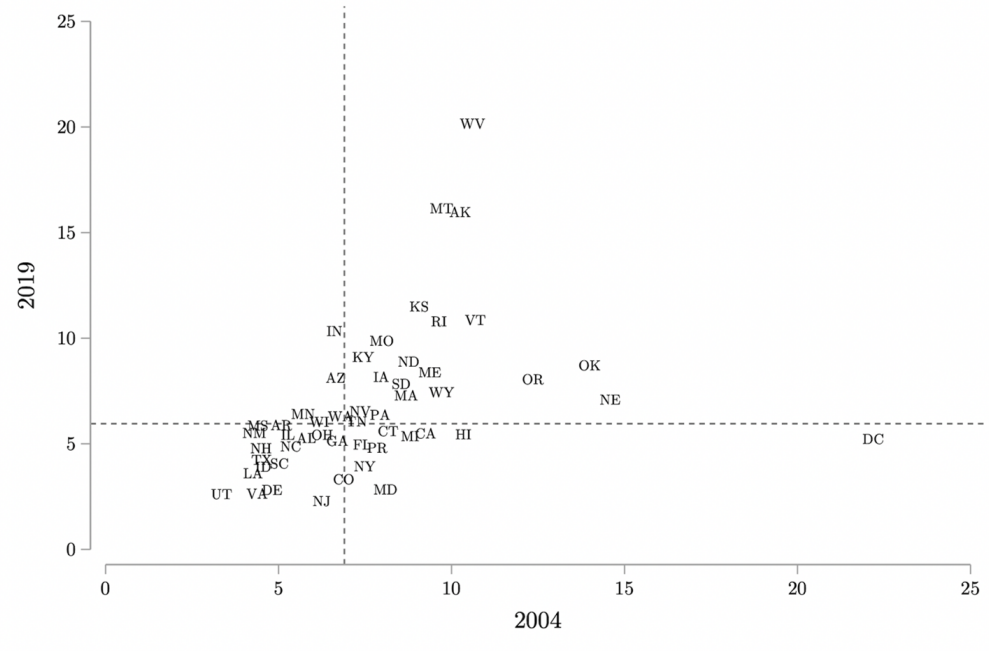
Notes: Parental substance use includes alcohol abuse and drug abuse. Removal reasons are not mutually exclusive since children can be removed for multiple reasons.

Figure C.4: *Children in foster care by most recent placement setting at end of year*



Notes: We report the percentage of children ages 0 to 17 who were in foster care at the end of the fiscal year and placed in each setting. Placement settings are mutually exclusive since we only observe the most recent placement. The "other" category includes pre-adoptive home, trial home visit, supervised independent living, and runaways.

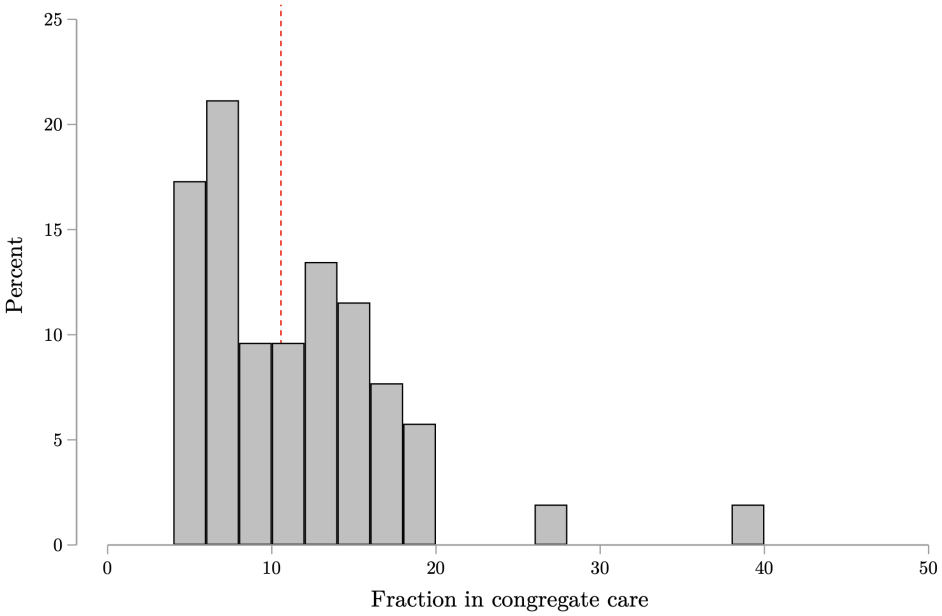
Figure C.5: *Children placed in foster care (rate per 1,000) in 2004 and 2019*



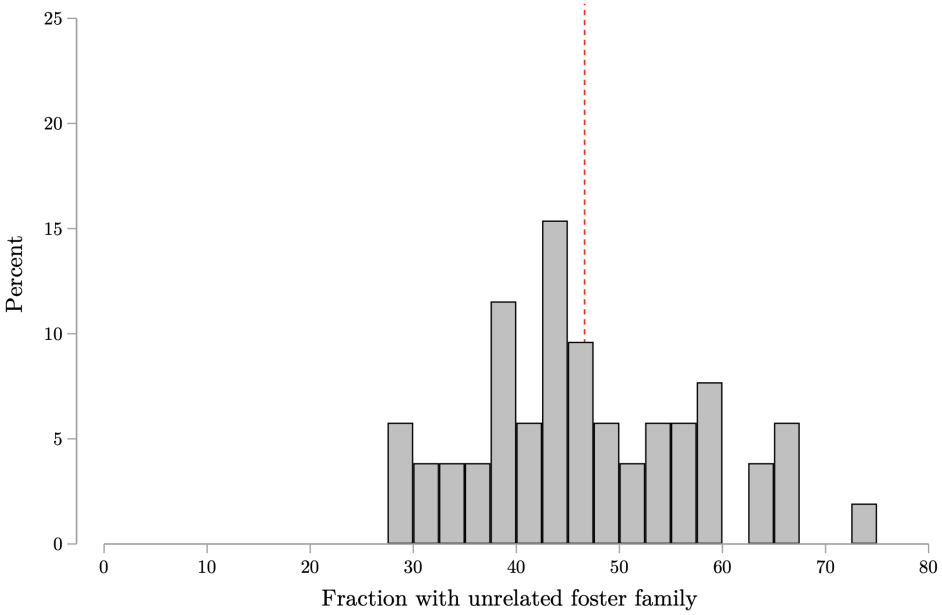
Notes: We report the rate per 1,000 children ages 0 to 17 placed in foster care at the end of the fiscal year. The vertical and horizontal dashed lines represent the median values in 2004 and 2019.

Figure C.6: *State variation in percentage congregate care and unrelated foster family*

(a) *Histogram of percentage of children in congregate care*



(b) *Histogram of percentage of children with unrelated foster family*



Notes: Panel (a) reports the percentage of foster children in congregate care and Panel (b) reports the percentage of foster children with an unrelated foster family (one observation for each state). We report the percentage of children ages 0 to 17 who were in foster care at the end of the 2019 fiscal year and who were placed in each setting. The vertical lines represent the nationwide percentage in congregate care and percentage with an unrelated foster family.

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